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Antiquity

A QUARTERLY REVIEW OF ARCHAEOLOGY

Edited by Glyn Daniel

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Antiquity

VOL. XXXIV No. 135 SEPTEMBER 1960

Editorial

WHEN ANTIQUITY was founded in 1927 our knowledge of the British Neolithic was thin and uncertain. The first edition of Childe's *Dawn of European Civilisation* and Kendrick's *The Axe Age*, both published in 1925, spoke of Wexcombe Ware: in the same year Menghin in his appendix to the third edition of Hoernes's *Urgeschichte der bildenden Kunst in Europa* talked of Grimstonkeramik. Four years before, Cyril Fox (who contributes an article of outstanding interest to the present number of ANTIQUITY) said that the Neolithic was ' an ill-defined dumping ground for stone implements (other than Palaeolithic and Mesolithic) not known by clear-cut associations with datable grave deposits to be of the early metal age.'

The work of Leeds and Childe prepared the way for the work of Stuart Piggott; his *Neolithic Pottery of the British Isles* appeared in 1932, and his *Neolithic Cultures of the British Isles* eighteen years later. In this book Piggott argued for the beginning of the British Neolithic at around 2000 B.C. and so it seemed to most people in the nineteen-fifties. Now radio-carbon dating has shown that this date was a thousand years out. We have already, in the last few numbers of ANTIQUITY, printed in notes and articles by Waterbolk, Giot and Watts the dates for the Neolithic in France and Ireland, and now we are happy to print here (p. 212) Dr Isobel Smith's note on the three dates for Windmill Hill itself determined by the British Museum Research Laboratory. It is now clear that the beginning of the Neolithic in north-western Europe belongs to the second half (or perhaps the fourth quarter) of the 4th millennium B.C.

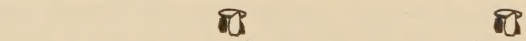
Incidentally, we have had to wait a long time for the publication of these Windmill Hill dates; perhaps the British Museum could devise a quicker system of releasing its Carbon-14 dates. It is good to know that when the Carbon-14 plant in the National Physical Laboratory is working its dates will be released as and when they are determined and not be delayed for the periodical meetings of a Committee. The Council for British Archaeology, in its report no. 10 for the year ending 30 June, 1960, in commenting on the recommendations of its Neolithic and Bronze Age Research Committee, says: ' The screening of specimens submitted to the National Physical Laboratory for Carbon-14 tests, and the speedier release of information on tests undertaken by the British Museum, has been urged by the Committee.'

There are still some archaeologists who are loth to accept Carbon-14 dates; Professor Milojčić of Heidelberg is perhaps the most vocative of them. Some point to the apparent gap of a thousand years between the archaeological and radio-carbon dates, but often on

examination this gap is an illusory one. We could say in Britain that there was a gap of a thousand years between the date of the Neolithic given in Piggott's *Neolithic Cultures* and the radiocarbon dates for the beginning of Windmill Hill, but this gap was due only to the fact that our archaeological dates before 1400 B.C. had no firm basis whatsoever. The same is true of Eastern Europe and in the next number of ANTIQUITY we will be publishing a very interesting article by Mr James Mellaart, Assistant Director of the British School of Archaeology at Ankara, dealing with the problem of the apparent gap between archaeological and Carbon-14 dating in Anatolia and the Balkans.

Recently Dr Harry Godwin, F.R.S., Professor-Elect of Botany in the University of Cambridge, and for many years Head of the Department of Quaternary Research gave the Croonian Lecture at the Royal Society—a masterly performance, demonstrating brilliantly how the dates for the late Quaternary and Holocene produced by clay varve counting were confirmed by Carbon-14 dating, and themselves confirmed it. When published, this lecture will be an indispensable summary for all concerned with prehistory. In a letter to the Editor Dr Godwin urges that all archaeologists should give the source of any Carbon-14 dates which they quote, and we ask that anyone writing for ANTIQUITY should do this, as Dr Isobel Smith has done in her note, using the accepted abbreviations for the Laboratories.

Meanwhile Willard F. Libby, who started Carbon-14 dating when he was in Chicago, and is now Professor of Chemistry in the University of California at Los Angeles, has given us a readable summary of the technique and method of radio-carbon dating in an article in the May, 1960 number of the *Journal of the Chemical Society*. It has, however, some curious statements which show that Libby, distinguished scientist though he is and originator of radio-carbon dating, is sometimes unable to appreciate the application of his work to human history, as when he says: ' We do not find any evidence of human beings in England before about 10,000 years ago. This is also the oldest American—the oldest Dane —the oldest Irishman, etc.—and they are all contemporaneous.' This is a devastating example of the divorce between science and the humanities which is so often written about.



We make no apology, even to our readers most far from the British Isles, for returning briefly to the problems of the Royal Commissions on Ancient and Historical Monuments discussed six months ago in the Editorial of our March number (ANTIQUITY, 1960, 1). In the first place, we have received a very considerable amount of correspondence on this subject. Secondly, *The Times* (21 March, 1960) devoted a two column article to the problem. Thirdly, the matter was raised in the House of Commons in an Adjournment Debate on Wednesday, 25 May (*Hansard*, vol. 624, no. 120, pp. 405–414).

A quarterly like ANTIQUITY can hardly have a lively correspondence, and the rapid and valuable give-and-take which goes on in the correspondence columns of the daily and Sunday papers is impossible here. But we must mention, editorially, some points from two of the many letters we have received about the Royal Commissions. The Director of Publications of Her Majesty's Stationery Office writes that ' the suggestion that the Inventories be issued in sections is certainly of interest. The Royal Commission on Historical Monuments (England) is, in fact, already considering the possible advantages of issuing their works in smaller, and therefore, cheaper, volumes.' The General Secretary of the Institution of Professional Civil Servants writes: ' It may not be known to you that the staff (*of the Royal Commission on Historical Monuments* (*England*)), through the Institution of Professional Civil Servants, gave evidence to a Committee of Inquiry domestic to the

English Royal Commission that sat in 1958. The Institution is very anxious indeed that there should be a speed-up in the publication of volumes because there is a developing frustration amongst the staff at this failure to secure publication. . . . The Institution has made specific suggestions that would streamline the efforts of these highly skilled individuals and throw rather more responsibility on the officers in the field making the surveys. At the present moment we are convinced that the procedure which involves to quite an extent a re-doing of the work of the field officers, holds up volume production to an enormous degree, and the improvements secured are certainly not worth the delay involved.'

The article in *The Times* was called ' Leisurely Progress of History in the Making: More Volumes Besought more Often ' and contained the results of an interview with the Secretary of the English Commission in which he is reported as having said, ' Now that we have got the measure of the prehistoric material and are being assisted by modern methods, we shall have a far higher rate of production. We are better equipped than ever before.' It is good to know that there are some official quarters where it is thought that the measure of prehistory has been obtained. The debate in the House of Commons was initiated by Mr Martin McLaren who revealed that the annual vote for the Commission was £50,000, and concluded that ' it may well be that the work of the Royal Commission would receive new impetus if the Commission was wound up as such and its functions transferred to the Ministry of Works, where they could be combined with the Minister's existing responsibilities.' Major Sir Frank Markham, supporting Mr McLaren, said he was ' appalled at the slowness of publication of the Royal Commission,' and added, ' My own estimate is that, considering the present pace at which the Commission is proceeding, it will take 240 years before it covers the British Isles.' The Financial Secretary to the Treasury, in replying, was sympathetic and informed. He was particularly sympathetic to the idea of publication in parts, and said that he was himself to be the Chairman of a Working Party to discuss the work of the Royal Commission, and ' to arrange any necessary reassessment and reorganization of the Government's various activities in this whole field.'

The existence of this Working Party is splendid news, and we wish it luck. It should consider all the activities of the state in relation to archaeology and our ancient and historical monuments. It will, of course, consider the possibility of grouping together the various activities at present carried out by the Royal Commissions, the National Buildings Record, the Archaeology Division of the Ordnance Survey, and the Ancient Monuments Department of the Ministry of Works. We believe that the Commission system is out-dated, and that we need a Central Archaeological Archive in a Department of State under some such official as the Queen's Antiquary, directly responsible—until we have a Minister of Fine Arts, or of Arts and Amenities—to the Minister of Works. We believe that, even if the Archaeology Division remains, it should be reformed by decentralization, especially by decentralization of editing, and the production of work by parishes or regions in fascicule form, each entry initialled by the investigator concerned. (To hell with the Civil Service tradition of anonymity; these entries are not policy minutes but scholarly records, and how can we attract scholars into our Commission staffs if they are to remain mute and inglorious while their colleagues in museums and universities can publish under their own names ?)

We shall publish with excitement the results of the Treasury's Working Party. It is good to know that there is such interest in the state in archaeology. We reiterate what we said six months ago: the British archaeological effort in state archaeology is so good in so many ways that we are all encouraged to try to make it better in certain ways. Our Ordnance Survey maps and the volumes published by the Royal Commissions since the last war are the cynosure of foreign archaeologists. This should not make us complacent. We must improve.

Under an improved and centralized organization there should be no need to half-apologize, as the Royal Commission on the Ancient and Historical Monuments of Scotland has done, for their admirable publication *The Stirling Heads* (H.M. Stationery Office, Edinburgh, 1960, pp. 20, 40 plates, 10*s*.). The Stirling Heads are a series of carved medallions which once formed the enrichment of the timber ceiling of King James V's Presence Chamber at Stirling Castle; they are one of the most remarkable and least known legacies of Renaissance craftsmanship in Britain and no detailed study has ever been made of them before this monograph. The Scottish Commission felt that adequate justice could not be done to these Heads in their forthcoming *Inventory of the Ancient and Historical Monuments of Stirlingshire*, and so prepared this illustrated account. Warmest congratulations to them and their staff and to the enterprise of the Scottish Stationery Office. There is a real demand for monographs of this kind, and if the Commission staffs were released from a too rigid county basis, we could have often small books like this one dealing with a special subject or a special area to the benefit of all. The only small publication the English Commission has attempted is the Guide to St. Alban's but we understand several other Guides were planned but never published. The Commission is working in Dorset, Wiltshire, Yorkshire and Cambridgeshire. We want little guides to Ely and York Minster and Avebury, for example, and we cannot wait 240 years for them. *The Stirling Heads* points the way, and Sir Edward Boyle's Working Party will, we hope, see that way.

Several correspondents have referred to our suggestion that there could be such an official as the Queen's Antiquary: by implication we were thinking back to the King's Antiquary. But has there ever been such a person in the British State ? The article on John Leland in the *Dictionary of National Biography* states: ' In 1533 Leland was made King's antiquary, an office in which he had neither predecessor nor successor.' In the *Warburg Institute Journal*, 1950, 313, Professor Momigliano discusses this statement and finds its first authority in T. Smith's 1691 *Life of Camden*. In his ' Newe Yeares Gyfte to King Henry the VIII ' called *The Laboriouse Journey and Searche for Englandes Antiquitees*, Leland states that in the thirty-fifth year of the King's reign he obtained a ' moste gracyouse commyssion to peruse and dylygentlee to searche all the lybraryes of Monasteryes and collegies of thys your noble realme ', and signed his pamphlet ' Joannes Leylandus Antiquarius '. Momigliano asks is there any authority other than this signature for supposing Leland was King's Antiquary, referring us to the opinions of E. N. Adams (*Old English Scholarship*, 1917, 12) and T. D. Kendrick (*British Antiquity*, 1950, 47). But even if we have no precedent, let us go ahead with the idea of a Queen's Antiquary in charge of a centralized archive, responsible for the protection of our monuments, the archaeological entries on our maps, and the publication of descriptions of our ancient monuments in quick, appropriate and readily available form. We commend this idea to the Treasury Working Party.

We wrote in the June 1960 Editorial of the death of Sir Leonard Woolley, and we print here (p. 220) a note by Professor Max Mallowan describing the Woolley Memorial Fund. We also print (p. 211) a note by Professor Emery on the UNESCO plan for Nubia. In previous comments on this matter we have said that we would try to provide information to guide the many who want to volunteer for work in Egypt and the Sudan, and to answer the many queries we have received on this matter. In a letter to the editor Professor Emery

says: ' I have made no mention of volunteers, for the hard fact is that we have a surfeit of them. What is needed now is money and gifts of equipment.'

As we go to press, the *Observer* publishes in its issue of 3 July an article by Professor L. R. Palmer, Professor of Comparative Philology in the University of Oxford with the extravagant title ' The Truth about Knossos '. The *Observer* also, in a piece of infelicitous and sensationalist journalism, common enough in some Sunday newspapers but fortunately rare in the *Observer*, headlines Palmer's contribution as ' Knossos Findings Misrepresented: Archaeological Sensation: Oxford Don's Discovery ', and then adds in two sentences which have been widely deplored by scholars (including Professor Palmer): ' Did Sir Arthur Evans, carried away by the splendour of his discoveries, unconsciously misreport the evidence ? Or has Professor Palmer exposed a conscious misrepresentation, reminiscent of the Piltdown Man fraud ?' These vulgar, startling catch-fivepenny words make one ask: How many guns can one jump ?

We shall return to the problems discussed by Professor Palmer in a later issue (meanwhile, the article we print in the present number by Mr Sinclair Hood on ' The Late Bronze Age *tholoi* of the Aegean Area ' is very relevant to the whole problem of Minoan-Mycenean relationships), *but only when his views are fully published*, and we deplore the fact that he has not given us his views in a scholarly and well-argued form before attacking Sir Arthur Evans in the necessarily compressed form of a newspaper article. We censure the *Observer* for its bad taste and for trying to make capital out of what in the end must be a complicated matter of conflicting interpretations by archaeologists and linguists. The parallel is not with Piltdown but with Rouffignac, where the truth (or otherwise) of an important discovery was wilfully subordinated to press publicity and sensationalism. These things do great harm to archaeology and scholarship and should be vigorously resisted. Compare the Palmer-Knossos and the Rouffignac affairs with the debunking of Piltdown Man which first proceeded by learned papers and lectures to learned societies, and widespread discussions in the scholarly world, before ever there were newspaper articles. It is worth remembering with pleasure at this moment the restraint, integrity and scholarly care with which Dr Oakley and Dr Weiner handled the publication of their archaeologically sensational discoveries. Incidentally Piltdown is in the news again. In a recent issue of *Nature* (July 9, 1960, p. 174), Dr Ashley Montagu argues that the Piltdown cranial bones were artificially thickened, and Dr Kenneth Oakley says that Dr Montagu's experiments while ' most interesting ', ' cannot be considered as providing a possible explanation of the unusual thickness of the Piltdown cranial bones.' In fact he questions that it is so unusual and says that in the British Museum collection there are other crania with walls just as thick as those of the Piltdown skull.

Tholos Tombs of the Aegean

by M. S. F. HOOD

Mr Sinclair Hood, Director of the British School of Archaeology at Athens, here discusses, at the invitation of the Editor, the origin of the Late Bronze Age Tholos *Tombs of the Aegean Area. A fuller publication of this material, with a detailed distribution map and catalogue of sites, will be published in a forthcoming issue of the* Annual of the British School at Athens.

AMONG the most impressive monuments of the earlier part of the Bronze Age in Crete are the great circular communal tombs which began to be built, notably in the Mesara plain but also in other parts of the island, before 2000 B.C., and flourished in use throughout the first half of the 2nd millennium. Similarly, the most magnificent surviving architectural creations of the Late Bronze Age in the Aegean area are the stupendous beehive or *tholos* tombs of the chief Mainland centres like Mycenae. Tombs of this type, with corbelled stone vaults sunk in the ground and approached by long entrance passages (*dromoi*), seem to appear for the first time in the Aegean about 1600 B.C., and reach their finest and grandest expression on the Mainland of Greece in the two centuries between 1500 and 1300 B.C.[1] A map of the Aegean area showing the distribution of these two types of tombs accompanies this article (FIG. 1).

At first sight there appears to be little in common between one of the early Cretan circular tombs of the Mesara type, ruined and destroyed as they all are (PLATE XVII (*a*)), and the greatest of the intact Late Bronze Age *tholos* tombs like the Treasury of Atreus at Mycenae (PLATE XVII (*b*)). But the difference is hardly less spectacular between the Treasury of Atreus and what is left of little Bronze Age *tholos* tombs like those recently excavated by Rhomaios at the provincial site of Vourvoura in the highlands between Sparta and Arcadia[2] (PLATE XVIII (*a*)). Tombs, however, like the Treasury of Atreus, built of finely squared masonry with chambers 10 m. or more in diameter, were exceptional. Most of the Late Bronze Age *tholos* tombs, like the majority of the early Cretan tombs, were built of rubble, rough slabs or lumps of undressed stone (PLATE XIX (*b*)); and their chambers were often some 5 m. or 6 m. in diameter (some were much smaller, as at Vourvoura), about the same size as the chambers in many of the early Cretan tombs.

On the face of it, therefore, it would seem natural to suppose that the Late Bronze Age *tholos* tombs of the Aegean were in some way descended from the early circular tombs of Crete. The appearance of *tholos* tombs on the Mainland of Greece in the 16th century B.C. would be merely one aspect of the great wave of Cretan influence which began to make itself felt there about that time. This Cretan influence is clearly displayed for instance in the contents of the royal shaft graves at Mycenae, which appear to antedate the construc-

[1] Very few of the Mainland *tholos* tombs can be dated before the beginning of the Mycenaean II period, *c.* 1500 B.C. The date for the Treasury of Atreus, the greatest of these tombs standing near the end of the important series at Mycenae, is probably in the region of 1350–1330 B.C. as suggested by Wace, *Mycenae: An Archaeological History and Guide* (1949), 119 f. That of *c.* 1250 B.C. proposed by Mylonas, *Ancient Mycenae* (1957), 88 is almost certainly too late.

[2] *Ergon*, 1954, 38 f.; 1956, 81 f.

Fig. 1. Distribution map of the tholos *tombs of the Aegean.*

○ *Circular tomb of the Mesara type of the earlier Bronze Age (Early and Middle Minoan period) in Crete.*

● Tholos *tomb of the Late Bronze Age with circular chamber and corbelled beehive vault.*

△ *Rock-cut tomb of the Late Bronze Age imitating the shape of a circular built* tholos *tomb.*

■ *Built tomb of the Late Bronze Age with rectangular chamber but roofed with a corbelled beehive vault like a* tholos *tomb.*

▲ *Built tomb of the Late Bronze Age with rectangular chamber and keel vault.*

⊕ Tholos *tomb of the Iron Age (Protogeometric and Geometric periods) with circular chamber and corbelled beehive vault.*

⊞ *Built tomb of the Iron Age (Protogeometric and Geometric periods) with rectangular chamber but roofed with a corbelled beehive vault like a* tholos *tomb.*

N.B. The double circles with a number indicate the number of tombs (if there is more than one) in the case of the Mesara type and other Bronze Age tombs.

The numbers of tombs are not given for the rock-cut tombs, nor for the Iron Age built tombs which often occur in large numbers in regular cemeteries.

tion of the first *tholos* tombs at that site. *Tholos* tombs, however, were already it seems being built by this time in the S.W. corner of the Peloponnese (Messenia), where Cretan influence was strongly felt as early if not earlier than in the region of Mycenae.

Apart from the early Cretan tombs it is difficult to suggest any other very obvious ancestors for the Aegean *tholos* tombs. In the far West, south Spain and Portugal, is a remarkable series of circular built tombs dating from the local Bronze Age, among them true *tholos* tombs which have survived more or less intact. But if, as seems likely, these are in some way connected with the equivalent tombs of the Aegean, the evidence points to their being inspired by Aegean models and not the other way round.[3] To the east, in Cyprus, is an isolated circular tomb built underground; but it is very small (the chamber less than 2·40 m. in diameter), and in other ways anomalous.[4] In any case it appears to date from the beginning of the Late Bronze Age (Late Cypriote I) about 1600 B.C., and may also therefore be inspired by Aegean models.

Nearer home, certain early tombs in one of the Cycladic islands, Syros, have been regarded as possible ancestors for the Late Bronze Age *tholoi*. But these tombs are very small, and occur in cemeteries of little built tombs of many different shapes, ranging from squares or rectangles through trapezes to rough circles.[5] The circular shape of some of these tombs is therefore incidental; and although they may have inward leaning sides, they are roofed with big slabs, and are really more like cist graves than corbel-vaulted *tholos* tombs.[6]

Another idea, that *tholos* tombs were developed from circles of shaft graves like those at Mycenae, is improbable enough in itself.[7] An argument against it is the fact that the earliest known *tholos* tomb on the Mainland of Greece (about 1600 B.C., see p. 166 *infra*) is in the Pylos area of the S.W. Peloponnese, where so far at least no examples of earlier shaft grave circles have come to light. On the other hand many round burial mounds near Pylos are assigned to the Middle Bronze Age (Middle Helladic) before 1600 B.C. And it can be argued that the later *tholos* tombs were simply mounds of this kind with circular built chambers inserted into them. But this still begs the question of how the beehive *tholos* chamber itself originated.

It has indeed been claimed that the Late Bronze Age *tholos* tomb in the Aegean was an original and inspired creation without ancestors. The possibility of this in a limited sense may be admitted; for there is reason to suppose that the transition from a tomb of the early Cretan type built largely above ground to an underground *tholos* was something in the nature of a leap, rather than a gradual evolution: and there is an explanation which could be put forward as to why such a leap should be made. The early circular tombs of Crete are all collective tombs used for the repeated burials (often hundreds in number) of a whole community. But some at least of the most typical of the Late Bronze Age *tholoi* are royal or princely vaults. The underground *tholos* might therefore be regarded as simply an adaptation of the early Cretan collective tomb for royal or princely burials. But in fact, as we shall see, the development of the underground *tholos* may have been something independent from and earlier than its use as a royal vault.

[3] Childe, *Dawn of European Civilisation* (1950), 272 f. Evans, ' Two phases of Prehistoric Settlement in the Western Mediterranean ', *Inst. of Arch. Bulletin*, 1955–6, 1 f.

[4] Sjöquist, *Swedish Cyprus Expedition*, I (1934), 570.

[5] Tsountas, in *AE*, 1899, 79 f., figs. 1–10. Tsountas says these tombs differed essentially both in shape and construction from the cist graves which he knew from the other islands!

[6] See the very full and careful description by Mylonas of the Early Bronze Age (Early Helladic) cemetery of A. Kosmas near Athens, where tombs with stone-built walls of comparable type merge into slab-lined cist graves. (Mylonas, *Aghios Kosmas* (1959), 64 f.).

[7] Mylonas, *Ancient Mycenae* (1957), 100 f.

That the Late Bronze Age beehive vaulted tombs of the Aegean were inspired by and developed from the early Cretan circular tombs is a view widely held by those well acquainted with both classes of monument ever since early circular tombs began to be found in the Mesara plain of south Crete from 1904 onwards.[8] But it is not a view universally held,[9] and there are difficulties real or imaginary in the way of connecting the two types of monument. These difficulties may be summarized under four main headings: (1) the gap in time between the two classes of monument; (2) the structural differences between them; (3) the differences in the use for which they were designed; and (4) the comparative lack of circular Late Bronze Age *tholos* tombs in Crete.

(1) *The Gap in time.* This objection, never a very strong one if all the available evidence was taken into account, has become still weaker with discoveries of the last twenty years Circular built tombs employed for collective burials had a long history in Crete, from the end of Neolithic if not into Neolithic times. They were being built and used extensively in the Mesara plain of south Crete (but also in other parts of the island!) during the Early Minoan period. When these tombs were first discovered in the Mesara plain at the beginning of the century it was indeed thought that they were essentially an Early Minoan form of tomb. But it has since become clear that such tombs were built and used in Middle Minoan times as well. Indeed many of the tombs (including some of the largest and best constructed) which were in the past ascribed to the Early Minoan period may well be of Middle Minoan date. A circular tomb undoubtedly dating from the Middle Minoan period was explored by the Germans at Apesokari in the Mesara during the war.[10] Another, belonging to a distinctly advanced phase of the Middle Minoan (M.M. II A, about 1800 B.C., or later), came to light at Knossos itself in 1957 (PLATE XVIII (*b*)).

Ever since the excavations by Xanthoudides before the First World War it has been recognized that several of these early Cretan circular tombs were still being used for burials at the beginning of the Late Minoan period after 1600 B.C.[11] The latest floor in the Middle Minoan circular tomb recently excavated at Knossos, for instance, was laid down at the very end of the Middle Minoan period or the beginning of the Late Minoan (M.M. III B–L.M. I A, *c.* 1600–1500 B.C.).

This means that circular tombs of the Mesara type were used, if not actually built, in Crete at the very time when the first beehive-vaulted *tholos* tombs were constructed. One of the earliest of these underground *tholos* tombs may well be that on the Kephala ridge at Knossos excavated by Hutchinson on the eve of the last war (PLATE XIX (*a*)).[12] Its construction has been assigned by the excavator to the end of the Middle Minoan or beginning of the Late Minoan period (*c.* 1600–1550 B.C.); but the arguments for so early a date are not conclusive.[13]

In the S.W. corner of the Peloponnese, however, at Koryphasion near Pylos, a small underground beehive tomb was excavated by Kourouniotis many years ago.[14] Blegen has

[8] E.g. Evans, in his Preface to Xanthoudides, *VTM* (1924). More recently, e.g. Platon, *AE*, 1945–7, 70 f., esp. 71, note 1.

[9] E.g. Persson, *Royal Tombs at Dendra* (1931), 144 f. Wace, *Mycenae: An Archaeological History and Guide* (1949), 119. Mylonas, *Ancient Mycenae* (1957), 99 f.

[10] Matz, *Forschungen auf Kreta* (1942), 13 f.

[11] Cf. Xanthoudides, *VTM*, 8.

[12] *BSA*, 51 (1956), 74 f.

[13] *Ibid.*, 76 f. The sherds from the cutting in which the tomb was built can only give a date *Post Quem*. But the tomb may nevertheless be an early one, although the fragments of ' Palace Style ' amphorae (*ibid.*, pl. 11 (*c*)) suggest that it was being used in Late Minoan II times, *c.* 1450–1400 B.C.

[14] *Praktika*, 1925–6, 140–1.

now published the clay vases from this tomb showing that their affinities are with pottery of the Middle Helladic period and the very beginning of the succeeding Mycenaean (Late Bronze Age) of the Mainland (*c.* 1600 B.C.).[15] In a general way these vases look very Cretan, and two of them in particular, an oval-mouthed amphora and a jug with a flat out-spreading rim, are of types excessively common in Crete but somewhat rare on the Mainland.[16] These, if locally made and not imported from Crete, were surely made by people among whom Cretan influences were strong at the time. This urges the question: if the pottery is imitating Cretan models, why not the tomb ?

(2) *Differences in structure.* These have always presented the chief obstacle in the way of admitting a connection between the early circular tombs of Crete and the later *tholos* tombs.

(*a*) It is an axiom that the early circular tombs of Crete were built more or less above ground, whereas the beehive vaults of the Late Bronze Age *tholoi* (both in Crete and on the Mainland) were sunk below ground for the greater part of their height. But the difference in this respect between the two groups of monuments is not really so sharp. For many, if not most, of the early circular tombs in Crete were in fact sunk, although only to a comparatively slight degree, in some sort of cutting in the ground. Moreover, while these early Cretan tombs were normally built in flat ground, a few were set in steep hillsides, cut back to some extent to receive them. Conversely, although the most famous and characteristic of the great *tholos* tombs of the Late Bronze Age, like the Treasury of Atreus at Mycenae, were buried deep in sloping hillsides, several (significantly, perhaps, the very early tomb dug by Kourouniotis at Koryphasion near Pylos was one!) were sunk in quite flat ground. Moreover, in this same S.W. corner of the Peloponnese a number of Late Bronze Age *tholos* tombs built entirely above ground have now been identified[17]; and in general these tombs built above ground appear to be among the earlier *tholoi* in the region.

(*b*) *Mounds.* Several of the *tholos* tombs on the Mainland have preserved traces of a mound which once covered the vault. Even where no traces of the mound are now recognizable it may be inferred that it once existed. Probably all the *tholos* tombs of the Late Bronze Age, whether sunk in the ground or built above it, were surmounted by a mound of some kind.[18] But no indisputable sign of a mound has been recognized in the case of any of the early circular tombs in Crete. This may be accidental, since under the conditions of intense erosion that prevail in the island all obvious traces of mounds made of loose earth or stones could have disappeared. But it may in fact be the case that the early circular tombs of Crete being built largely above ground, were not covered with mounds. When the vault came to be set underground a mound of some kind was necessary to dignify the tomb.

(*c*) *Roofing systems.* The *tholos* tombs of the Late Bronze Age had stone corbelled vaults, in profile like an old-fashioned beehive, rising to a more or less steep point in the centre. The height of the vault was normally about equal to the diameter of the circular chamber. Many of these vaults have survived more or less intact (PLATE XVII (*b*)). Where only the lower parts of the walls have survived (PLATE XIX (*a*)), it is inferred that they once carried

[15] *Hesperia*, 23 (1954), 158–62.

[16] *Ibid.*, 161, pl. 38, 8 and *a*. Furumark, *The Mycenaean Pottery* (1941), 603, type 117, lists examples of this type of jug from the Mainland. For Knossos, see Evans, *PM*, I, 556, fig. 404 f.: ' The most numerously represented of all the types found ' in the Temple Repositories of *c.* 1600 B.C. Cf. *ibid.*, fig. 415B, from the same horizon in another part of the Palace.

[17] E.g. Those excavated by Valmin at Malthi (Valmin, *The Swedish Messenia Expedition* (1938), 207). For others, cf. Valmin, ' Tholos Tombs and Tumuli ', in *Acta Inst. Romani Regni Sueciae*, II (1932), 216 f.

[18] This was the opinion, for instance, of Kourouniotis in *Praktika*, 1925–6, 140. Cf. Dörpfeld, *AM*, 33 (1908), 298, apropos of the *tholos* tombs at Kakovatos.

similar stone-built vaults. But none of the early circular tombs of Crete have been found with their walls standing more than a metre or two high (PLATE XVII (*a*)).

The question of how the chambers of the early Cretan tombs were roofed is therefore open to debate, and has been much debated. An early circular tomb of an average size may have a chamber about 5 m. in diameter. But many of the known tombs are considerably larger than this, culminating in the giant tomb A at Platanos in the Mesara, whose chamber over 13 m. in diameter challenges comparison with that (diameter 14·50 m.) of the largest of the Late Bronze Age *tholos* tombs, the Treasury of Atreus itself.[19]

The three tombs at Platanos were built virtually free-standing in flat ground. How then was so large a chamber as that of Platanos tomb A roofed ? The idea that these early circular tombs in Crete were in fact unroofed is hardly to be entertained.[20] The rich grave goods in many of the tombs, and the depth of deposit often found covering the burial level in them despite the erosion of centuries, argue some kind of roofed chamber.

A more serious suggestion is that the early Cretan tombs had flat roofs. But flat roofs in the larger tombs at any rate would have required internal supports, for which there is not the slightest evidence. Indeed the great fires, of which traces have often been noted inside these tombs, lit either for ritual purification or with the very practical object of quelling the stench from the decaying corpses by burning aromatic substances, are hardly conceivable in chambers with flat ceilings, which (if they existed) must have been comparatively low, supported by wooden pillars and cross-beams.

The simplest way to roof a large circular space without using supports is by means of some kind of dome or vault. That this was in fact the way in which the chambers of these early tombs in Crete were roofed is suggested both by their circular shape and by the inward lean often visible in their walls where they are preserved to any height. Incidentally this inward lean of the walls may be as great or greater than the inward lean in the corresponding height of wall in many a Late Bronze Age *tholos* tomb![21] Above all, the immensely thick walls, which are such a regular and extraordinary feature of the circular chambers of these early Cretan tombs, would have no point unless they were meant to support the weight and thrust of a vault or dome.

In the case of the little early circular tomb at Krasi,[22] excavated by Marinatos some thirty years ago, it appeared probable that the chamber had been roofed with a low dome or vault built in stone. But at Vorou,[23] where owing to the remoteness of the site it seemed impossible that any of the original stones of which the tombs were constructed could have been removed for building houses or field walls in later times, Marinatos was forced to conclude that the larger of the two tombs at any rate could never have been roofed with a stone-built vault. The larger therefore of the early circular tombs in Crete may have had their walls built only to a certain height in stone, the upper parts of the walls rising to the vaults being continued in mud or mud-brick. These vaults of mud or mud-brick might have been strengthened with some sort of framework of wooden beams.[24] Such a method

[19] *VTM*, 88 f.

[20] As suggested by Seager, *Trans. Penn. Univ.*, II, 131.

[21] See Xanthoudides, *VTM*, 7. At Kalathiana, where part of the wall of the chamber is preserved to a height of 2 m. above the floor, the inward lean is as much as 0·24 m. or 1 in 8.

[22] *ADelt*, 12 (1929), 102 f.

[23] *Ibid.*, 13 (1930–31), 137 f.

[24] For discussions on roofing systems, e.g. Marinatos, *ADelt*, 13 (1930–31), 168 f. Rodenwaldt in *Gnomon*, 5 (1929), 179, note 2. The idea, often repeated, that it would have been ' impossible ' to span the chambers of the largest of these early Cretan tombs with free-standing vaults was based upon the assumption (e.g. Xanthoudides, *VTM*, 91) that such vaults must be built in stone. But in any case it is dangerous to be dogmatic about what the engineers of early times could or could not do.

of building, with the lower part of the walls in stone and the upper parts in mud or mud-brick with a framework of wooden beams, would be closely comparable with the system of construction in the palaces and great houses from *c.* 2000 B.C. onwards in Crete. When the vault was built underground as in the Late Bronze Age *tholos* tombs it would become more possible (and also necessary!) to construct it entirely of stone. Ideal reconstructions of ' typical ' forms of the two types of tomb are here shown (FIG. 2).

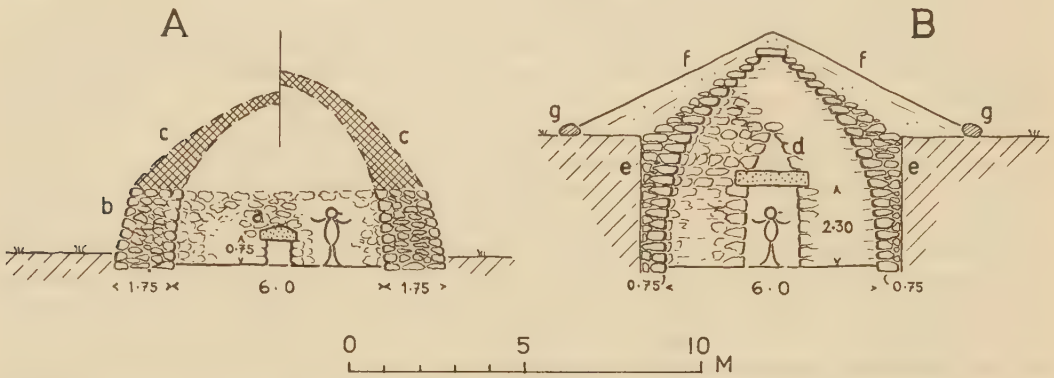


Fig. 2. Ideal reconstruction of ' typical ' (A) early Cretan circular tomb, (B) Late Bronze Age Tholos *tomb, of the same internal diameter. Key:— (a) Lintel over doorway ' humped ' to take weight of wall above. (b) Inward batter of walls on outside. (c) Vault of mud or mud-brick (two possible heights of vault). (d) ' Relieving triangle ', or space left above lintel, to relieve it of weight of wall above. (e) Walls built in cylindrical cutting in the ground. (f) Mound of earth or clay covering top of vault. (g) Ring of stones round base of mound. The ' man ' is 5 ft. 6 ins. (1·68 m.)—tall for an early Cretan at least.*

Why were these early tombs in Crete of this shape? As ' houses of the dead ' do they perhaps reproduce some primitive type of dwelling that had gone out of fashion among the living? It is true that no early circular houses that can be compared with them have yet been found in Crete, where even during Neolithic times the houses appear to have consisted of one or more rectangular rooms, presumably (to judge from later analogies) with flat roofs.[25] But in the lowest Early Bronze Age (Early Helladic) levels at Orchomenos on the Mainland of Greece a number of circular buildings were identified,[26] and sections of what may have been an enormous circular building were found in a corresponding horizon below the Late Bronze Age palace at Tiryns.[27] The largest of these buildings at Orchomenos was about 6·00 m. in diameter inside; and they had stone foundations with vaults or domes of mud-brick rising from them.

Marinatos has suggested that the round buildings at Orchomenos were in fact not houses but granaries.[28] But this was certainly not the case with the circular structures explored by Dikaios in the early Neolithic settlement of Khirokitia in Cyprus.[29] These were dwellings,

[25] For instance, the Late Neolithic houses under the central court of the Palace at Knossos (Pendlebury, *AC*, 39 f.; Evans, *PM*, II, 5 f.), and the house assigned to the Middle Neolithic phase at Katsamba north of Knossos recently excavated by Alexiou (*Praktika*, 1953, 305 f; 1954, 369 f.).

[26] Bulle, *Orchomenos*, I (1907), 19 f.

[27] Müller, *Tiryns*, III (1930), 80 f.

[28] Marinatos, *BCH*, LXX (1946), 337 f.

[29] Dikaios, *Khirokitia* (1953), *passim*.

and it is interesting to note that they were built with domes of mud-brick raised upon high stone foundations. The largest of these '*tholos*' dwellings at Khirokitia has an internal diameter of over 5 m., exceeding that of the chambers in many of the early Cretan tombs. What is most significant is the fact that in this still very primitive stage of culture people were simply buried beneath the floors of their circular dwellings. The early circular tombs of Crete are therefore merely survivals of primitive houses of the type found at Khirokitia, retained as dwellings for the dead after they had been abandoned by the living in favour of more convenient forms of habitation.

Mud-brick vaults raised upon stone foundations were not confined to Cyprus. In a settlement of the Neolithic Tal Halaf culture at Arpachiyah in North Mesopotamia Mallowan and Rose found similar large circular chambers, between 4 m. and 7 m. in diameter inside, with stone foundations and traces of mud-brick vaults.[30] These were in the centre of the settlement, and were not apparently used for burials; but whether they were dwellings in the ordinary sense of the word, or for what other purpose they were used, was not obvious.[31]

These architectural analogies for the early circular tombs of Crete in Cyprus and North Mesopotamia become all the more impressive when the many other affinities, in pottery for instance and sealstones, between the earliest Minoan (Bronze Age) civilization of Crete and the early cultures of the Near East are taken into account.

(*d*) *Entrances.* The great *tholos* tombs of the Late Bronze Age have lofty portals (PLATE XIX (*b*)). And even in the smallest of the Late Bronze Age *tholoi* the entrance is usually at least high enough for a man to walk through it without stooping. But in the early Cretan tombs the doorway was characteristically low, a hole just large enough for somebody to crawl through it and drag or push a body into the chamber. It does, however, seem that in the later examples of these circular tombs of the Mesara type in Crete the doorway (where any traces of it survive) tends to be higher, as if developing in the direction of the high entrance of the Late Bronze Age *tholoi*. Thus in the late example of a circular tomb recently excavated at Knossos the doorway, to judge from the surviving height of the jambs, stood at least 1·50 m. high; and this was an unusually small tomb with a chamber only 4·0 m. in diameter!

Most interesting in this connection was the pair of tombs excavated by Xanthoudides at Drakones in the Mesara.[32] One of these (△) appears to have been of the ordinary Mesara type: but the other (Z), built almost touching (△), was a hybrid with features connecting it both with the early circular tombs of Crete and with the Late Bronze Age *tholoi*. This tomb (Z) had a chamber over 7 m. in diameter with the walls standing in places to a height of over 2 m. The entrance, as seems to have been the case in all the early circular tombs of Crete, faced towards the east. But unlike the entrances usual in these tombs, it was a real doorway, 1 m. wide and 2 m. high: and instead of the little walled pit, often found outside the entrances of the early Mesara tombs, this doorway opened upon a rectangular anteroom 2·50 m. long and 1·30 m. wide, a kind of abbreviated version of the entrance passage or *dromos* of a Late Bronze Age *tholos*. Tomb (Z) continued in use into Late Minoan times,

[30] *Iraq*, II (1935), 25 f., esp. 33. The mud-brick vaults of these buildings at Arpachiyah appear to have been quite low. It is very possible that the vaults of the early Cretan tombs were in general lower in relation to the size of the chamber than the beehive vaults of the late Bronze Age *tholos* tombs.

[31] The '*tholoi*' cited by Evans from Libya (*PM*, II, 36 f.) were really round flat-topped cairns piled above graves, and 'probably late in date, although certainly pre-Islamic' (Bates, *The Eastern Libyans* (1914), 246 f., esp. 248).

[32] Xanthoudides, *VTM*, 76 f.

and during that period the walls of the circular chamber were strengthened in places by the construction of an inner skin. Incidentally, this strengthening would hardly have been necessary if the walls were not supporting some kind of vault, since they were already about 2 m. thick!

(*e*) *Dromoi.* The long entrance passage or *dromos*, such a prominent feature of the Late Bronze Age *tholos* tomb, has no place in the early circular tombs of Crete. For the long entrance passage is very much an attribute of the vault sunk below ground; it becomes necessary for approach to an underground chamber. An entrance passage was also of course necessary where the chamber, although built above ground, was covered by a large mound.

(3) *Differences in use.* There is one obvious and fundamental difference between the early circular tombs of Crete and the grandest of the Late Bronze Age *tholos* tombs like the Treasury of Atreus at Mycenae. The former were collective tombs used by a whole community for repeated burials over a long period of years, while the latter were royal vaults for the interment of kings or princes. Such royal or princely tombs would naturally attract plunderers in all ages, and only one, at Dendra near Mycenae, has been found more or less intact.[33] Here there were three burials, of a man and two women, apparently the king and his queen with a princess or retainer. The evidence suggested that they had all been buried in the tomb at one and the same time. But many of the smaller and simpler *tholoi* appear to have been family tombs used for repeated burials after the manner of the early tombs in Crete. And even some of the great *tholos* tombs may have been family vaults of this kind.

In the S.W. part of the Peloponnese (Messenia), where very large numbers of *tholoi* are known, several of those excavated have proved to be such family tombs. Indeed the very early *tholos* explored by Kourouniotis near Pylos, to judge from its small size and the simple character of the clay vases from it, may have been one.[34] Another *tholos* tomb with a large number of burials, recently excavated near the Palace of Nestor at Pylos, is interesting for comparison with Crete.[35] For some of the burials in this tomb had been placed inside large store jars or *pithoi. Pithos* burials of this kind have never apparently been recorded before now from a *tholos* tomb on the Mainland of Greece. But burial in *pithoi* was a common practice in Crete throughout the Middle Minoan period from *c.* 2000 B.C. onwards, and *pithos* burials of that period have been found in some of the early circular tombs like those at Vorou.[36]

(4) *Rarity of Late Bronze Age* tholos *tombs in Crete.* It is often thought that the Late Bronze Age type of underground *tholos* tomb is largely confined to the Mainland of Greece and does not occur in Crete. As late as 1939 Pendlebury could describe the tomb at Agios Theodoros near Gournia as ' the only true *tholos* of the Bronze Age yet found in Crete.'[37] This was not strictly true even at that time, since there was no good reason to suppose that the tombs noted by Hogarth at Knossos,[38] or by the Italian explorers, Taramelli and Halbherr, at Kamares and Erganos,[39] were not Late Bronze Age and circular *tholos*-

[33] Persson, *The Royal Tombs at Dendra* (1931).

[34] But it is said to have produced small fragments of silver vases and of a large pyxis of Egyptian faience (*Hesperia*, 23 (1954), 159).

[35] Blegen, *AJA*, 62 (1958), 178.

[36] *ADelt*, 13 (1930–1), 137 f.

[37] *AC*, 243. But later he suggested that even this *tholos* might be Iron Age (*BSA*, XXXVIII (1937–8, published 1940), 111, 138, note 3).

[38] *BSA*, VI (1899–1900), 81.

[39] *AJA*, V (1901), 270, 439.

vaulted.[40] Since Pendlebury wrote a number of Late Bronze Age *tholos* tombs have been found in Crete, including the very interesting and comparatively early example on the Kephala ridge at Knossos itself.[41] None of these Cretan tombs is on the scale of the great ' royal ' beehive vaults of the Mainland; but they are widely scattered throughout the island, and many of them occur in those very parts of East Crete which were afterwards the home of the native Cretans (Eteocretans), with their knowledge of a pre-Greek language preserved right into Classical times.

But circular *tholoi* were not the only kind of tomb being built during the Late Bronze Age in Crete. In this respect Crete presents a marked contrast with the Mainland of Greece, where the only form of vaulted tomb in the Late Bronze Age was the circular *tholos*.[42] In Crete, however, side by side with circular *tholos* tombs we find square or rectangular tombs surmounted by a *tholos* vault, and rectangular tombs rising to a keel vault. This variety in the forms of the vaulted tombs of Crete during the Late Bronze Age, contrasting with the uniformity of the Mainland, offers a possible line of argument in favour of the view that the Late Bronze Age type of *tholos* tomb originated in Crete.

It is true that many more Late Bronze Age *tholoi* are known on the Mainland, and that the largest and grandest of those known are there. But this does not in itself prove that the underground *tholos* tomb was first evolved on the Mainland rather than in Crete. When determining the centre from which some race of animals or plants has spread, the important factor is not where that race eventually flourished with the greatest exuberance, but where the largest number of species or varieties of it are to be found. So in this case, what may be significant is not the fact that there are many more *tholos* tombs on the Mainland than in Crete, but that in Crete there is a greater variety of different types of vaulted tombs, whereas on the Mainland only circular *tholos* tombs occur.[42]

Little *tholos* tombs continued to be built and used into the Iron Age, the Protogeometric and Geometric periods, in various parts of Greece. But in two areas, Thessaly and Crete, these Iron Age *tholoi* appear to be most at home. Above all, in Crete cemeteries of little *tholos* tombs are regularly found attached to the settlements of people who, with a degenerate form of the Minoan (sub-Minoan) culture, had taken refuge in the mountains before invaders, whether the Dorian Greeks or others, who were reaching the island at the end of the Bronze Age. Many of these Iron Age *tholos* tombs, like some of the Late Bronze Age *tholoi*, occur in the Eteocretan region of East Crete.

Conclusions. The early circular tombs of Crete were built above ground. The larger of them at any rate appear to have had domes of mud or mud-brick, although some of the smaller may have been vaulted in stone. They were collective tombs used by a whole community for repeated burials over a long period of years. The *tholos* tombs of the Late Bronze Age were normally constructed below ground, and were probably always vaulted in stone. Unlike the early Cretan tombs, they were usually at any rate covered by a mound. Some of the Late Bronze Age *tholoi* were royal or princely vaults, but many appear to have been family tombs used for repeated burials.

Some of the later examples of the early Cretan tombs display features like a high entrance reminiscent of the Late Bronze Age *tholoi*. But the sinking of the vault below ground may have been the result, not of gradual evolution, but of a ' leap '. Such a ' leap ' might have

[40] For Knossos see *BSA*, VI (1899–1900), 81. The tombs at Erganos and Kamares were arbitrarily assigned by Pendlebury to the Protogeometric period (*AC*, 306, 314–5). But the pottery from those at Erganos was certainly Late Minoan (Desborough, 251, 325); that from the Kamares tombs is now unknown (*ibid.*, 259, 325).

[41] *BSA*, 51 (1956), 74 f.

[42] Except for rare examples of elliptical vaulted tombs, at Thorikos (*ADelt*, 1890, 159 f.; *Praktika*, 1893, 12 f.; *AE*, 1895, 221 f.) and Palaiochori (*ADelt*, 9 (1924–5), Par. 18; *BSA*, 51 (1956), 168). The shape at Thorikos at any rate appears to be conditioned by the narrow ridge along which the tomb is built.

taken place when it came to adapting the early circular vaults of Crete as royal tombs. There is nothing, however, to prove that the earliest underground *tholoi* were royal tombs and not family vaults.

The earliest examples of underground *tholoi* on the Mainland have been found in the S.W. part of the Peloponnese (Messenia). This area of the Mainland early came under a strong influence from the Minoan civilization of Crete, and the idea of the circular vaulted tomb may have been introduced here from Crete about 1600 B.C. whether by actual colonists or by Cretan merchants living in the coastal towns. For these coasts would be directly on the route of Cretan ships trading with Italy and the West, and importing, for example, the liparite obsidian from the Aeolian islands, which was being used for making fine stone vases at the great centres like Knossos in still earlier times.[43]

Was then the underground form of the circular vaulted tomb actually developed or invented in this part of the Mainland rather than in Crete ? If here on the Mainland, was it developed by Cretan settlers or by the natives themselves under the influence of Cretan ideas and skills ? The mounds that cover the Late Bronze Age *tholos* tombs might be taken to reflect a local Mainland tradition. For burial under mounds (but not in vaulted chambers!) was common in Messenia during the Middle Bronze Age.[44] On the other hand, piling a mound to dignify an underground tomb is an obvious enough idea.

Whatever the answers to those problems, the early circular tombs of Crete and the underground *tholos* tombs of the Aegean area in the Late Bronze Age must be connected. Both classes of monument belong to a single family even if they have individual characteristics.

ABBREVIATIONS

AC	Pendlebury, *The Archaeology of Crete* (1939)	*BSA*	*Annual of the British School at Athens*
ADelt	*Arkhaiologikon Deltion*	*Ergon*	*To Ergon tes Arkhaiologikes Hetaireias*
AE	*Arkhaiologike Ephemeris*	*PM*	Evans, *The Palace of Minos*, I–IV (1921–35)
AJA	*American Journal of Archaeology*	*VTM*	Xanthoudides, *The Vaulted Tombs of Mesara* (1924)

[43] Evans, *PM*, I, 87, II, 56, describes a large unworked lump of this material from a deposit of the Middle Minoan II phase in the Palace at Knossos (*c.* 1750 B.C.). This is illustrated by Mosso, *The Dawn of Mediterranean Civilisation* (1910), 363, fig. 198. A fragment of a fine vase of liparite came from a deposit of Middle Minoan III A (*c.* 1650 B.C.) (*PM*, I, 412). The superb vase in the form of a shell from A. Triadha near Phaistos is assigned by Evans to the transitional period Middle Minoan III B—Late Minoan I A, *c.* 1600 B.C. (*PM*, II, 823).

[44] E.g. The Middle Helladic burial mound at Agios Ioannes near Papoulia in the Pylos area excavated by Marinatos (*Praktika*, 1954, 311 f.).

Book Chronicle

We include here books which have been received for review, or books of importance not received for review, of which we have recently been informed. We welcome information about books, particularly in languages other than English and American, of interest to readers of ANTIQUITY. *The listing of a book in this chronicle does not preclude its review in* ANTIQUITY.

HELLENISTIC CULTURE: Fusion and Diffusion. *By* MOSES HADAS. *pp.* 324. *New York, Columbia University Press; London, Oxford University Press*, 1959. 35*s.*

A GUIDE TO PREHISTORIC ENGLAND. *By* NICHOLAS THOMAS. *pp.* 268 *including* 69 *figures and photographs. London, Batsford*, 1960. 30*s.* A gazeteer arranged by counties of the most important and most interesting prehistoric remains in England. To be used in conjunction with Jacquetta Hawkes's *Guide to the Prehistoric and Roman Remains of England and Wales*, and the Ordnance Survey maps of *Ancient Britain*. It is a pity that at the moment when we are all beginning to discard the classification Neolithic, Bronze and Early Iron Age, Mr Thomas still uses it, although, with disarming frankness, finds it unusable in Cumberland.

[*continued on p.* 206.

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Archaeological Argument: Some Principles*

by R. M. COOK

In this short article the Reader in Classical Archaeology in the University of Cambridge discusses some of the methodological problems of archaeology. Often talked about by prehistorians, these major issues of interpretation and inference are seldom discussed by classical archaeologists.

CLASSICAL archaeology has a bad name with many prehistorians, partly because of its preoccupation with aesthetics, partly because it regards material remains as subsidiary to literature. But even if the exponents of Greek archaeology appear amateurish, their subject has a theoretical value for prehistoric studies. Excavation has been extensive and written records often provide checks on conclusions that might be suggested by the material evidence. I give some examples.

1. INVASIONS

According to tradition and the distribution of dialects much of southern Greece was conquered and occupied by the Dorian branch of the Greeks about the time of the transition from the Bronze Age to the Iron Age (roughly the 11th century B.C.). The *Iliad*, most of which had been composed from older materials by the 8th century, is set in the Mycenean Period (that is the Late Bronze Age) and its political geography ignores and apparently antecedes the Dorian invasion. Among later writers the Dorian invasion was accepted as an essential part of history. As for the linguistic evidence, the Linear B tablets from Pylos and Mycenae show that in the Late Bronze Age an Arcadian dialect of Greek was spoken in the south-west and north-east Peloponnese, but by the 7th and 6th centuries B.C. (when our earliest relevant inscriptions begin) Arcadian dialects were restricted to the mountainous interior of the Peloponnese and to Cyprus, which had been colonized by Greeks in the Late Bronze Age: the easiest explanation of the change is again an invasion of Doric-speaking Greeks between the 12th and 7th centuries B.C. But although it is eighty years since archaeologists began looking for relics of the invading Dorians, they have not found them. It is tempting to assign to the Dorians the destruction of the Mycenean palaces and higher civilization, and yet this revolution is not limited to the parts of Greece conquered by the Dorians. The introduction of iron, the Geometric style of pottery, and changes in burial rites have at one time and another been attributed to the Dorian invaders. But if, as seems certain, the Dorians came from north-west Greece, it is very unlikely that they were the first Greeks to obtain iron; the Geometric style of pottery developed without any break from Mycenean and its leading school was at Athens, which according to tradition and dialect was not conquered by Dorians; and the change in the customs of burial seem to have no special significance, as will be argued in the next paragraph. There is in fact so far no material evidence that is certainly characteristic of the Dorian themselves. It seems that the invaders, who may have been a pastoral people, travelled light, and being more primitive than the peoples they conquered took over their basic arts and crafts. Here, then, is an example of an important invasion that has left no positive archaeological record.

* Professor J. M. Cook and Mr G. E. Connah were kind enough to read and criticize this paper.

An opposite type of conclusion comes from the examination of graves of adults in the Greek Iron Age. The best series is from Athens,[1] a state that Greek tradition asserts repelled the invaders. In the Mycenean period, which ends about 1100 B.C., inhumation was regular: groups (presumably families) had rock-cut chamber tombs, in which the bones of the last interment might be swept aside at each new burial. During the Submycenean period (1100–1025 B.C.) there was a change to single burials and to cremation, and this practice continued throughout the Protogeometric period (*c.* 1025–900 B.C.—the earliest phase of the Iron Age) and well into the Geometric period (900–700 B.C.). Inhumation returned about 800 B.C. About 700 B.C. cremation became more usual. In the 6th century inhumation was regular once again. These are not the only changes in burial customs at Athens between the 11th and the 6th centuries. Yet so far as we know—and for the later part of this span we know enough—there was no invasion of Attica nor even any considerable immigration. The conclusion must be that a radical change in the customs of burial is not necessarily evidence of a change in population or, indeed, of anything more than a change in fashion.

2. RELIGION

Fashion again had some part in the type of offerings made in Greek sanctuaries, and it is dangerous generally to argue from offerings to beliefs. At Olympia, for instance, a principal sanctuary of Zeus and the site of the Olympic games, the normal offering of good quality around 700 B.C. was a bronze figurine of a horse or an ox. Yet there was no cult of a horse or ox deity, or even of a deity specially connected with those animals. In turn we may consider the religious beliefs of the Mycenean period, when small female figurines were very popular. These figurines are sometimes cited as evidence that the Myceneans worshipped a dominant mother goddess. But the Linear B tablets show that Zeus and other members of the Olympian Dynasty were already established.[2]

To return to graves, Greek literature and epitaphs show a general disbelief in any happy after-life. Homer, for example, in the Geometric Period regards death as virtually final; the ghosts of the dead flit insubstantially about Hades, barely aware of anything and only temporarily brought back to lively consciousness by magic practices. Yet Geometric graves are furnished quite well with weapons, jewellery, and pots for drink and drinking. Grave goods, it seems, were an almost meaningless survival from earlier beliefs, or perhaps a sentimental or competitive tribute to the dead, of the sort that insists on brass handles for coffins. It is worth noting that in 6th century Athens a law was made restricting display at funerals.[3] Evidently no strong religious principle was involved.

The various strata of Greek religious belief are not at all equally represented in the archaeological remains. Besides the public cults of the Olympian gods and of other, partly assimilated deities, there was in ancient Greece a persistent belief in private magic. Hesiod in the 8th or very early 7th century gives good examples. But the archaeological evidence for magic is negligible, especially in the earlier periods of Greek civilization. There is even less archaeological evidence for Greek philosophical thought.

3. PROSPERITY

The historian Thucydides, who was working in the later 5th century B.C., took care to

[1] K. Kübler in *Bericht über d. VI Internat. Kongress für Archäologie*, 428–30. For the evidence (and revised conclusions) see *Kerameikos*, I, IV, V and VI.

[2] M. Ventris and J. Chadwick, *Documents in Mycenaean Greek*, 125–7.

[3] Cicero, *de Leg.*, II, 59 and 64. Plutarch, *Solon*, 21.

point out that material remains are not an automatic index of prosperity. He was arguing, curiously enough, that because the ruins of Mycenean Mycenae were small in extent it was not right to deny that Mycenae itself had been powerful. As proof of this statement he compared the archaeological potentialities of Sparta and Athens in his own time. Similarly there is a great contrast between the remains, particularly the architectural remains, of the late Anglo-Saxon and early Norman periods in England: yet there cannot have been much difference in total economic prosperity. Here there is a lesson for Classical students. It is asserted time and again that the Early Iron Age in Greece was a Dark Age, greatly impoverished in comparison with the Mycenean period. From the Mycenean civilization we have the palaces or rather castles (which Thucydides thought unimpressive), tholos tombs, wall-paintings, intricate metalwork, and a detailed accountancy. These were all manifestations of a strong and centralized autocracy, and it is not surprising that when that autocracy disappeared they disappeared too. In the Early Iron Age power was passing to a landed aristocracy, none of whose members could afford or even needed the higher arts of the old palace régime. But total prosperity—a concept which needs careful definition—may have remained constant. Indeed the introduction of iron, which was useful for tools as well as for weapons, suggests that the means of production must have improved; for if iron was not in some way advantageous, why was it used? These arguments might be considered in comparisons of prosperity in other regions during their Late Bronze and Early Iron Ages.

4. TRADE

From the 8th and 7th centuries B.C. onwards objects of Greek manufacture are found widely spread in the Mediterranean countries and beyond. The biggest class of these objects is fine pottery, for the obvious reason that pottery survives most chemical reactions in the soil and has almost no value as scrap. This pottery has been studied carefully and most of it can be dated fairly closely and assigned to its place of origin. It is clear that in foreign markets the pottery of Corinth was much the most successful from the late 8th to the early 6th century and that afterwards Athens obtained an almost complete monopoly. But it would be dangerous to conclude that pottery is an index of general trade, or even to suppose that Corinthian and Athenian traders in turn dominated the carrying trade.[4] Aegina, for instance, is known to have been a trading city of importance, but since it made no high-class pottery there is no archaeological evidence of its activity. Even in the Archaic and Classical world much trade must have been indirect; in less advanced societies the likelihood of indirect trade is greater, especially when the surviving objects of trade are few. Students of early Mediterranean civilization, for example, seem to be much too credulous about the ' tin trade ' and its organization, and perhaps even the ' amber route ' may be delusive.

In conclusion it appears to me that many archaeologists, both Classical and Prehistoric, are not critical enough when they try to reconstruct a society from its material remains, and further that they tend to exaggerate the significance of particular objects. It is a valuable corrective to consider the material remains of historical peoples and to test archaeological methods of deduction in problems where the answer is already known.

[4] The economic conclusions to be drawn from Greek pottery are discussed more fully in *J.d.I.*, 1959, in proof. To complicate the problem many Classical students assert that the small, narrow-mouthed Corinthian pots were exported full of scent, but there is no good evidence for this.

Megalithic and Other Art: Centre and West

by T. G. E. POWELL

Mr Powell, Senior Lecturer in Archaeology in the University of Liverpool, here discusses the art of certain monuments in Central Germany, and the question of its affinities with art elsewhere in prehistoric Europe.

THE occurrence of designs cut on the stone walling slabs of certain prehistoric tombs in Central Germany has led from time to time to a search for comparisons, even prototypes, in the mural art of megalithic tombs in Atlantic Europe. Within recent years, some new observations and discoveries of considerable interest have been made principally as a result of a re-excavation at the well-known Lohne (Züschen) megalithic tomb near Fritzlar, and through the excavation of a tumulus on the Dölauer Heide near Halle/Saale (FIG. 1).

The first of these monuments is a large, megalithically constructed, collective tomb with a long rectangular chamber entered through a port-hole slab which is set between the main chamber and a short ante-chamber. The whole monument is set in a trench so that the roof was approximately at ground level. This is one of a group of such tombs in Hesse; there is a neighbouring group in Westphalia, and these are related both to a group in Sweden, and, more particularly, to the classic tombs of this type in the Paris Basin (SOM). The Dölau tumulus is something quite different, for in it, amongst other things, has been found a stone-constructed grave for only one individual. The wall and cap-stones are of moderate size but not megalithic, and the grave can be likened in various respects to some three other stone-built graves, all in the Halle district, and of which the best known was that found at Göhlitzsch with its representations of bow, quiver, and battle-axe, cut, with other decoration, on the wall faces.

The cultural, and even chronological, distinctions that might appear to separate Lohne (Züschen) and Halle/Dölau are complicated by the fact that both share a particular decorative motif: close-set pick-executed chevroning, and, further, this is rather different from anything found in mural art in the far west.

Lohne (Züschen) also displays schematic representations of horned animals yoked to sledges or carts, also not known in the west, but recalling some rock carvings in the Ligurian Alps. In fact, Lohne displays two kinds of mural art not known in any tombs of the Paris Basin group to which it is structurally related, nor anywhere in Atlantic Europe. Furthermore, there is nothing in the burial ritual, or material culture, of the Dölau/Göhlitzsch graves, themselves but an aspect of the Saale Single Grave/Corded Ware (*Saaleschnurkeramik*) Culture to suggest that any influence had flowed eastwards from the cultures of the megalithic tomb builders in the far west assuming, indeed, that western chronology can allow a sufficiently early date for such a possibility. How, in either of the two cases under discussion, is the idea of sepulchral mural art, whether for ritual or decorative ends, to be accounted for? Has too much been made of apparant western similarities? The problem has, however, a further extension for at least one unequivocal *statue-menhir*, of the southern

Fig. 1. Location of decorated monuments in Central Germany.

French sort, has been found close to the Saale, and that used as building material in a grave of Early Bronze Age type in that region.

Finally, account has to be taken of ordinary menhirs, or large standing stones, for these are widely distributed in Western and Central Germany, with a small group in Bohemia, and these monuments also have some claim for consideration in the present context.

It becomes clear that an explanation of the interplay of all these factors in Central Ger-

many is no straight forward matter. The present notes are offered not so much as a solution of these problems as an attempt to clarify certain aspects of the available material, none of which is as well known to students in the west as it deserves to be. The writer has been so fortunate as to have been able to examine the Dölau and Göhlitzsch stones in the Landesmuseum at Halle/Saale, having already had some acquaintance with megalithic art in Atlantic Europe.[1]

A beginning may be made with the tomb at Lohne (Züschen), and Dr Otto Uenze's excellent published descriptions and commentary merit close attention.[2] Here only some of the more salient points can be mentioned. At least some of the megalithic slabs, whether for walling or roofing, had had to be transported from distances of about four kilometres, and although capstones are rare at all the Hesse tombs, chiefly because of their removal in later times for agricultural or building purposes, Uenze makes the interesting point that wooden roofs may also have been employed on account of the scarcity of stone. The results of excavation of the tomb at Lohra, Kr. Marburg, where no capstones were in position, suggests that the decay of a wooden roof had allowed the low surmounting tumulus to collapse into the chamber and lie directly on the upper burial deposits.[3]

The burial deposits at Lohne (Züschen) were removed in 1894, but very satisfactory evidence comes from the excavation of a similar *steinkammergrab* at Altendorf, Kr. Wolfhagen, in which about 250 inhumations had taken place.[4] In all tombs of the Hesse group, as elsewhere, pavements of small flagstones had been laid on the chamber floor, and between various burial deposits as these rose from floor level. Skulls were often laid along the walls, and groups of bones enclosed in stone settings. These contrivances were for the purpose of making more room in the chamber for subsequent interments, and integrated skeletons near the entrance, but within the chamber proper, presumably represent the final use of the tomb by the community who erected it. Neither in Hesse, nor in Westphalia, is there evidence for any common ceramic tradition amongst the tomb builders, nor for pottery being an integral element in their material culture at all. Fragments of imported collared-flasks of the First Northern, Funnel-Beaker (TRB), Culture come from the Lohne (Züschen) and Altendorf tombs, but there are no rough pots with S-profile and modelled foot of the SOM kind, and Uenze makes the important suggestion that these pots in the Paris Basin should represent a late stage in the use of those tombs when the local inhabitants had come under the influence of intruders of the Single Grave (*Einzelgrab*) Culture, who possessed distinctive footed vessels. This point could best be resolved by more precise observations during excavations of Paris Basin tombs than has been the case heretofore. Furthermore, there is no known occurrence of Grand Pressigny flint in the *steinkammergräber* of Hesse or Westphalia, but this flint does occur in Single Grave associations in these regions. The SOM-pot/Grand Pressigny flint horizon in the Paris Basin tombs should therefore be later than the primary use of the Hesse-Westphalia tombs. Both at Lohne (Züschen) and at Altendorf, sherds of true Single Grave pottery came to light, and the

[1] My particular thanks are due to Dr Hermann Behrens for photographs of two of the Dölau stones here illustrated, and for facilities provided at Halle. To Mr J. L. Forde-Johnston I am indebted for the drawings in FIG. 2.

[2] Uenze (1956) and (1958). See Select Bibliography at the end of this article. This tomb is at Lohne, not at Züschen, which is a neighbouring town. I have therefore followed Dr Uenze's usage. I am indebted to Dr Uenze for various kindnesses, and for his stimulating writings on the subject.

[3] *Kurhessische Bodenaltertümer*, III (1954), 27–48. Dr Daniel informs me of a completely wood-built tomb in the Paris Basin group: Bonnières-sur-Seine, *v. Bull. Soc. arch., hist. et scientifique de la région de Bonnières-sur-Seine* (*S.-et-O.*), 1953, 17–28.

[4] *Kurhessische Bodenaltertümer*, III (1954), 5–26.

exact excavation technique employed showed that these sherds occurred only in the deposit immediately below the roof.[5] Intrusive Single Grave burials in these tombs may be likened to the way in which many megalithic tombs in Atlantic Europe were put to secondary use by the makers of Bell-Beakers, and in Britain and Ireland this practice continued to the time of Bronze Age ' food-vessels ' whose makers were descended, at least in part, from Bell-Beaker and Corded Ware ancestors.

The art at Lohne (Züschen) may now be considered. Uenze has drawn attention to the horned animal art occupying the central position on the wall faces of such stones as are decorated in this manner.[6] The designs certainly appear to represent a ' vertical view ' of horned animals, probably oxen, that, in a number of cases, are yoked in pairs or threes, and draw some kind of vehicle (FIG. 2 (a)). This vehicle is represented as a straight line drawn at right angles to the pole, each end terminating in a deep pit that might be interpreted as a wheel. In a few cases there is a semi-circular line drawn to join these pits crossing over the pole. It must again be a matter of conjecture as to whether these represent the body of some kind of vehicle, and, as Uenze points out, whether this art has any inference for the transportation of some of the blocks from a distance. A possible connection with rock art in the Ligurian Alps has already been mentioned,[7] but these animals at Lohne (Züschen) should not be confused with the simple ox horn designs found in some of the megalithic tombs in Brittany although they may well have a common Mediterranean ancestry as proper subjects for portrayal in ritual art.[8]

The close-set chevroning, or herring-bone (*fischgrätenverzierung*) at Lohne (Züschen) is executed in deep pick technique, and occurs only near the upper edge of two stones. Uenze very rightly draws attention to the close connection of this motif, both in pottery and grave decoration, with the Single Grave Culture, and deduces that its appearance at Lohne is in connection with the secondary use of the tomb when only the upper portions of the walling stood free. This chevron style in tomb art is best exemplified on the walls of Grave 7 in the tumulus excavated by Dr Hermann Behrens on the Dölauer Heide near Halle/Saale, in 1953.[9] This tumulus is of particular interest for a number of reasons. It overlay a long rectangular post-built house of the Salzmunder phase of the Funnel-Beaker (TRB) Culture, and itself contained a wedge-shaped area, bounded by ditches, which together with several graves, all appeared to belong to this phase of the Funnel-Beaker Culture. The stone-built, decorated, grave (no. 7), with an enlarged tumulus covering, were additions. The approximate measurements of the grave are: length 4 m., breadth a little over 1 m., and internal height 1 m. The walls comprise eleven stones, and the roof consists of six rather narrow slabs.

The proportions of the stones hardly allow the grave to be described as megalithic. At the inner end of the grave, and occupying about a third of the floor space, there had been constructed a wooden platform or floor. Apart from the actual remains of wood, the earth was darker in this area than elsewhere. Some disturbed bones of a man's skeleton were found scattered about the whole floor so that his original position cannot be determined, though the majority of such bones as survived lay in the centre of the grave, not at the

[5] Uenze (1956), 77–88, for further refs. and discussion.

[6] Uenze (1958), 100–2, draws attention to small stone slabs, found loose in the tomb deposit, with carvings. See our fig. 2 (a), from a slab of this kind which displays the detail of the animal art better than do any of the wall stones.

[7] C. Bicknell, *The Prehistoric Rock Engravings in the Italian Maritime Alps*, Bordighera, 1911, especially pl. III. Also: M. Louis, *Les gravures préhistorique du Mont Bego*, Guide Sommaire, Paris, n.d., fig. 2.

[8] Péquart and Le Rouzic, *Corpus des signes gravés . . du Morbihan.* (1927), especially pls. 24, 46, 50.

[9] Behrens, Fasshauer, and Kirchner (1956) for original study of the decorated stones. Behrens (1958 c) for excavation report on the tumulus, while (1958 a and b) are convenient summaries.

Fig. 2. Selected motifs from Central German monuments.

floored end. There were no grave goods, but seven of the eleven wall stones were highly decorated; five had designs executed by the use of a sharp, deep-cutting pick, more possibly a punch used with a hammer-stone, while on one stone the decoration had been effected in white colouring, and on another both picking and painting had been carried out. The good condition of the painted designs, and the tenacity of the material used makes it improbable

that the stones with pick-executed designs only had lost an original application of colour through atmospheric conditions.

For detailed description of this Dölau grave the original reports must be consulted, but here the first point to be noted is the general over-all nature of the wall decoration, especially at the inner end of the grave. Both painted stones (nos. 5 and 8) are here, one of them (5) being the end stone (PLATE XX (*b*)). This end stone displays a general white painted scheme of a linear design in which hatched triangles and prominent reserved vertical zig-zags are conspicuous. Also portrayed are five axes standing vertically as if placed against the wall. These were picked and then painted after the linear scheme had been completed. Whatever may be thought of the significance of the other decoration at Dölau, the whole impression conveyed by this stone is the same as that of the Göhlitzsch slabs: it is the inside of the man's house with his weapons conveniently at hand.

The white-colour decoration on stone 8 is of the same pattern, reserved zig-zags, and hatched triangles, as the background scheme on stone 5. Coming to the stones with picked decoration, the inner face of stone 2 is entirely covered with a chevron pattern in rather widely spaced picked lines, and stone 3 shows on the upper half of its face a simple linear pattern of horizontal and oblique lines. On the lower part of the face a large and shallow vertical groove had been picked out, and this may have had something to do with a timber which formed the outer edge of the wooden structure.

The two stones that perhaps lend themselves most to controversy are stones 4 and 10 (FIG. 2, *b* and *c*). Both stones have much rougher natural surfaces than the other slabs employed, and in both cases decoration is confined to an area covering more or less the upper half of the wall face. The main design on stone 4 consists, from the lower edge upwards, of three rows of alternating oblique lines set within a framework of single horizontal lines, and above these is a series of short vertical lines with some other less distinct picking including at least one small circle. Two horizontal lines terminate the upper boundary of the whole scheme. The most conspicuous feature on this stone is a more carefully pick-executed design which occupies the greater part of the left-hand side (as viewed) of the decorated panel. This problematical design has been usefully referred to by ProfessorHorst Kirchner as the Oval (*das* '*Eirund*'), and this describes its general shape very well. The Oval is some 30 cm. in height, and 5 cm. wide. The upper end is more pointed than the lower. The surface within the enclosing line is covered with a simple chevron design of picked lines except at the upper end where there are three horizontal lines. Depending from the enclosing line, where it curves upwards from the broad base on the outer side, are some ten short lines with broader ends that look like a row of tassels. It would be possible to discuss this design at length, but it must suffice to say that while the views so far published tend to see in it a version of some anthropomorphic motif represented in western Passage Grave art, the present writer believes it to be a depiction of some kind of object, possibly a bag, or container of amulets, but of some sort that might have been normal in the dead man's house just as were his weapons. Neither in execution, nor in detail of design, does he see any real likeness to West European megalithic art.

Stone 10 displays a pair of conjoined pentagonal frames in each of which is a 'pine branch' that has again been interpreted as a borrowing from western tomb art. The 'fir-tree man' is an uncommon, and evidently late, motif in a few Irish Passage Graves, and except for its basic linear construction, shares nothing in appearance with these balanced motifs at Dölau, nor with the smaller but comparable 'pine-branch' close to the axe-blade on one of the Göhlitzsch slabs.[10] A possible interpretation, here put forward, is that these Central

[10] Behrens, Fasshauer, and Kirchner (1956), pl. III, and frequently illustrated elsewhere. The position of the handle near the butt of this axe, as in the Dölau examples, suggests that metal forms were intended.

German motifs represent pelts, or dried skins of animals stretched on frames, hung on the walls, and so, with the other patterns whether representing textiles, painted plaster, or interwoven wattling, contribute to portray the interior decoration of a contemporary house of the living. It remains to mention stone 9 (PLATE XX (*a*)) for it displays a straightforward mural pattern in which the rows of close-set chevrons are almost identical with the ' secondary ' chevrons at Lohne (Züschen). The discovery of this stone 9 at Dölau provides strong support for Dr Uenze's view as to the origin of the chevron art at Lohne, and similarly the two isolated stones from Ellenberg, Kr. Melsungen,[11] in Hesse, may be attributed more certainly, as Uenze suggested, to the westward expansion of the Single Grave people.

In connection with the use of colouring matter at Dölau, it should be said that the stones of the Göhlitzsch grave, when found, were coloured in red, black, and white. Some red colouring is till to be seen embedded in the picked lines. Another grave of the Saale Single Grave/Corded Ware Culture with coloured walls was that found at Stedten, but here the four walling slabs were specially selected from different rocks: red sandstone, white limestone, porphyry, and conglomerate.[12]

Additional to Dölau and Göhlitzsch, two other stone-built graves in the Halle region are known to have possessed incised mural decoration. These were found at Nietleben, and Schopkau, but they present special problems. In the first place the decoration is of a fragmentary, indeed perfunctory, kind, but sufficient in its content to show its reliance on the art style exemplified at Dölau and Göhlitzsch. Secondly, the graves were distinctly wedge-shaped, the entrance being at the narrower end, and thirdly, both graves contained pottery distinctive of the Bernburg group which falls late in the Funnel Beaker (TRB) sequence, but which is generally regarded as ante-dating the appearance of *Saaleschnurkeramik*. In the inner part of the Nietleben grave, traces were found of a wooden platform or floor, and this feature would seem to provide a strong additional link with the rites and practices of the builders of Grave 7 in the Dölau tumulus. These points are all emphasized by Dr Behrens who believes that a distinction should be kept in mind as between the decorated graves containing Bernburg pottery and those, as yet without pottery, which display battle-axes and Corded Ware style motifs, on their walls. The chronological difficulty cannot be properly explored here, but it may be suggested that either the Bernburg pottery continued sufficiently late to be contemporary with the earlier phase of the Saale Single Grave/Corded Ware Culture, or that the perfunctory decoration at Nietleben and Schopkau was the result of influence from funerary practice through external contacts before the actual settlement of Single Grave people in the Halle region. Whatever the exact process, it may be noted that the pottery from the two graves in question is reported to belong to Bernburg II/III and II respectively.

It has already been proposed that in execution, content, and arrangement, the art of these Halle graves is something quite different from that of megalithic tombs in Atlantic Europe. Before attempting a brief statement of the different nature of the western art, something must be said as to an alternative origin for that found in the Halle graves. It is not necessary to put forward any thing novel, for it has long been recognized that a tradition of ' house-graves ' was of great antiquity, and of long endurance, throughout the grasslands lying north of the Caucasus, and, so far as Europe is concerned, extending well into the Pontic region north of the Black Sea.

These ' funerary parlours ' were more often wood built with floors and foundations of

[11] Uenze (1956), pls. 44 and 45, and (1958), 105, fig. 7.
[12] V. G. Childe, *The Danube in Prehistory* (1929), 149.

small stones, than constructed with stone slabs, but slab built graves of the Globular Amphora (*Kugelamphora*) Culture are well known, and provide reasonable, and chronologically proximate, proto-types for the Halle graves under discussion.

Although no mural decoration has been reported from graves of the Globular Amphora Culture, the importance of its bearers for the initial transmission of ideas, and aspects of material culture, across the grasslands from the south-east has been fully recognized.[13] It is from the ' dolmens ' of the western Caucasus that comes direct evidence for both painted and incised mural decoration, but unfortunately there appears to be nothing more definitive on this subject since Tallgren's invaluable summary of the old explorations which he published in 1934.[14] Tallgren compared some of the linear ornament to textile patterns, and suggested a connection between the Caucasian graves and Göhlitzsch, as well as with the later princely, wood-built, tombs of Leubingen and Helmsdorf. Recently, Dr Marija Gimbutas has demonstrated clearly the relationship of the various central and north European grave forms, associated with the Globular Amphora and Single Grave/Corded Ware Cultures, to the Kurgan Culture of the Ukraine, and thence back to the parent culture of the Kuban-Terek and Caucasian region. Within the compass of the Kurgan Culture, the Hut-grave phase provides the closest analogies for the Central European graves. The Hut-graves, themselves, show direct evidence for wall hangings in the form of patterned rugs as in kurgan 7 at Tri Brata near Elista, north-east of the Caucasus. The fashion for mortuary comfort, however, had already been set before the close of the 3rd millenium B.C. by such regal personages as the principal occupant of the great Maikop kurgan.[15]

From settlement sites, wall coverings of organic material have of course not survived, but decoration on plaster surfaces is well attested from a number of prehistoric cultures in central and eastern Europe. Childe drew attention to painted plaster at Lengyel and Ottitz, and at Arişud in the rectangular houses of the second phase.[16] In Hungary, houses of the Tiza Culture, excavated by Banner at Kopàncs, had plaster covered walls with incised, and red and white painted, geometric designs.[17] The incised panels of geometric ornament on the inner wall of the house model from the Tripolje settlement at Popudina may also be noted.[18] More directly relevant to the present interest is the wall plaster painted in bands of rose-red, whitish-yellow, and grey, from the Tumulus Bronze settlement at Jiřikovice in Moravia.[19] This culture having undoubted Single Grave/Corded Ware antecedents.

In seeking to compare the phenomenon of mural decoration as noted in certain Central German graves of various cultural associations with the art found in some groups of megalithic tombs in Atlantic Europe, it is quite insufficient to deduce connections on the strength of one or two approximations in motif discovered in individual western tombs. It must always be enquired where the tomb, and its mural art, may stand in relation to other tombs of the same group, and whether in the light of stylistic, geographical, and chronological, considerations, the art of a chosen western tomb might possibly represent a situation when this phase of the art could have been propagated in an easterly direction. So far as the present writer can see, the drawing of parallels between west and centre in this matter has been too largely the result of a mere hunt for individual motifs without regard to context,

[13] M. Gimbutas, *The Prehistory of Eastern Europe*, I (1956), 140–57 with bibliography.

[14] *Eurasia Septentrionalis Antiqua*, IX (1934), 1–46.

[15] M. Gimbutas, *op. cit.*, Hut-graves, 74–80; Maikop, 58.

[16] V. G. Childe, *op. cit.*, 88 and 99.

[17] *Praehistorische Zeitschrift*, XXI (1930), 184–93. I am indebted to Professor Stuart Piggott for this reference, as also for much valuable discussion on the whole subject. The plaster is now exhibited in the museum at Hódmezövásárhely.

[18] M. Gimbutas, *op. cit.*, fig. 56, 2.

[19] Seen in Brno, M., brief report in *Archeologické rozhledy*, I (1949), 161–63.

much less to precision in the things compared.[20] It is perhaps not sufficiently understood by many students of the subject that there is no general, over-all, family of western megalithic art. Such art as exists is highly peculiar to certain groups of tombs only, and each group has its own art style both in range of subject matter and presentation. Only in the most general way can the various groups be linked together, and that more through the recognition of a common ancestral tradition than through the sharing of precisely similar characteristics.

So far as Ireland is concerned, the art of the Boyne Culture Passage Graves is now known largely to represent a stage in disintegration of an originally coherent, but schematic, anthropomorphic style, so that ' fir-tree men ', zig-zags, and concentric arcs, amongst other patterns, can be recognized as stemming from more elaborate and explicable compositions.[21] It is unjustifiable, therefore, to seek to relate these random motifs in Ireland with the Central German grave art. In any case, the possibilities in simple geometric design, as witnessed in prehistoric Europe generally, are so limited that recurrences over wide areas cease to have that kind of significance so often claimed.

In Brittany, the position about art in megalithic tombs is altogether more complicated. Here there is a predominant group of decorated Passage Graves, centred in the Morbihan, but with outliers. There are some tombs in the province, within the classification of Gallery Graves, that display examples of ' mother-goddess ' art comparable to that found in some of the Paris Basin tombs and in some of the rock-cut tombs of the Marne-Petit Morin group.[22] The art of these tombs is unlike either of the two styles at Lohne (Züschen), and does not enter directly into the present discussion. In Brittany it is the art of the Passage Graves that is of present moment. In these, the schematized tomb-goddess is an important subject of portrayal, but the stylistic treatment is different than in Ireland, and would appear to derive more directly from a Mediterranean source parental also to the style of the southern *statue-menhirs*, and so with some affinity to the anthropomorphic figures of the Paris-Marne tombs. The Passage Graves in Brittany are also remarkable for some tombs with elaborate over-all curvilinear designs, as in Gavr'Inis, and for pictographic symbols which may variously represent horns or ships, shrines, reaping or cultivation tools, and certainly hafted axes. In Gavr'Inis, the high relief sculptured axes, without hafts, clearly represent the well-known ritual jadeite axes. Only in the tomb of Petit Mont, now destroyed, were there walling slabs which showed markings that might possibly be compared to the chevron designs in Central Germany. The execution of the design was altogether less exact, and was looser and more broken in composition. These could hardly have stood as a prototype, and, on other ground, it may not be altogether inopportune to sound a word of caution as to the chronological position of such tombs as Petit Mont. Need its erection have been much earlier than the mid-2nd millennium B.C., and what may have been the significance of the perforated stone battle-axe reported from its excavation ? It would not be so impossible as to wonder whether, in the western expansion of some elements, the Single Grave Culture did not make an eventual contribution to tomb art on the shores of the Atlantic. A date for all of the Halle graves within the first quarter of the 2nd millennium B.C., if not earlier, is much more probably established than the date of any decorated Passage Grave in Ireland or Brittany.

[20] This is the chief fault in an otherwise valuable compendium of information by Dr Waldtraut Schrickel: *Westeuropäische Elemente im Neolithikum und in der Frühen Bronzezeit Mitteldeutschlands*, I (1957). The West European claims are nearly all to be denied.

[21] T. G. E. Powell and G. E. Daniel, *Barclodiad y Gawres* (1956), 41–55, for discussion, with bibliography, of Boyne Culture tomb art.

[22] G. E. Daniel, *The Megalith Builders of Western Europe* (1958), 89–104, for France, with bibliography. Péquart and Le Rouzic, *op. cit.*, for illustrations of Morbihan tomb art, including Gavr' Inis and Le Petit Mont.

With regard to technique in treating the stone surface, it should be noted that the most usual method in the west was to produce a relatively broad line of rounded pock-marks by the use of a hard hammer stone, probably a hand held pebble. The intended design was in fact not cut or punched on to the stone surface, but battered. There are, of course, some variations in technique in the west, and in some few instances a metal tool may have been employed. Painted designs are known with certainty only in western megalithic tombs south of the Pyrenees. In the north-west of the Iberian Peninsula, there are some few Passage Graves with both painted and pock-executed designs, and it is from this source that the Boyne Culture tombs almost certainly received their anthropomorphic art. Perhaps at Pedra Coberta, the highly stylized anthropomorphic pattern was extended to provide an overall scheme of true wall decoration, but this cannot be claimed with any certainty.[23]

It remains to comment briefly on the other two kinds of stone monument in Central Germany mentioned at the beginning of this essay: a single *statue-menhir*, and a large number of plain standing stones, the ordinary kind of menhir.

The *statue-menhir* (FIGS. 1 (1) and 2 (*d*) was found at Pfützthal, Kr. Eisleben, in use as a cover slab for a grave that should fall, on indirect evidence, within the end of the local Neolithic or beginning of the Early Bronze Age.[24] The grave is unlikely to have been later than the early phase of the local facies of the Aunjetitz Culture.

It may reasonably be assumed that this stone had performed an earlier function as a free standing cult figure in the manner of the classic southern French group. The anthropomorphic design consists of a very simple nose and mouth with necklace of four roughly concentric arcs. Further down the face of the stone are traces of some five arrangements of small oblique lines forming single chevrons. The rock is a fine-grained sandstone, and the design was apparently cut as to make continuous incisions, not either punched or pocked. The face and necklace of the Pfützthal stone show its close affinity to the classic French group, but whether its iconography was carried to the banks of the Saale from the Lower Rhône, or whether it came from a possible source in northern Italy, by way of the Alps, no opinion can here be ventured.[25] In view of the use of two *statue-menhirs* as building material in the roof of the Passage Grave at Collorgues, Gard, and that the economic background of the builders of this tomb appears to have been Early Bronze Age, the cultural background of the Pfützthal stone could well have been in the opening phase of the Aunjetitz Culture. Its chronological position vis à vis the art of the Halle graves must therefore be subsequent. The Pfützthal *statue-menhir* is probably not unique in the Saale region, but the carvings on the slab from Dingelstedt, Kr. Halberstadt, and on the menhir at Seehausen, Kr. Wanzleben,[26] are respectively too indeterminate, and too divergent to merit discussion here.

For a full treatment of unadorned menhirs, reference must be made to Professor Kirchner's fine monograph.[27] He points out that, in themselves, such menhirs can provide no direct archaeological evidence, and that the motives for their erection may have been many and equally valid over long periods of time. There are in fact examples of menhirs standing on both Bronze and Iron Age tumuli, but there are also large numbers unassociated with any known grave or ritual setting. Kirchner has thought it well to say that these appear to represent the spread of an idea, a ritual practice, and not the spread of a particular culture, in the ordinary archaeological use of the word, of which they might have been one manifes-

[23] Powell and Daniel, *op. cit.*, 50, collect references to painted tombs in Portugal, Galicia, and Asturias.

[24] Kirchner (1955), 14, citing comparanda, and pl. XXIX.

[25] Kirchner (1955), pl. XXX, illustrates two of a number of decorated stones from Merano, Trentino, of close affinity to *statue-menhir*, and some Breton Passage Grave, art. See also: M. Ornella Acanfora, *Le Statue Anthropomorfe dell'Alto Adige*, 1953.

[26] Kirchner (1955), pl. XXII (*c*), for Seehausen. Schrickel, *op. cit.* Katalog, 76–78, for Dingelstedt.

[27] Kirchner (1955), *passim.*

tation. Simple menhirs are of course also well known in parts of Belgium, in northern France, and to a lesser extent in western Britain and Ireland. Again, indications for date point to a wide range in time, but in Brittany one or two menhirs, not seemingly part of a larger monument, can be shown, by random motifs of Passage Grave art cut on their flanks, to be at least as early as some Passage Graves.

As to whether the practice of erecting free-standing menhirs derives from the use of such stones in more complex settings, such as the menhir in the long tumulus at Manio, or in the Carnac alignments, no answer can here be attempted, but menhirs are unlikely to have been substitutes for megalithic tombs as ritual monuments. The kind of stone to be handled, with its method of erection, was generally different. In Belgium, De Laet has pointed to the similar distribution of menhirs and the SOM Culture.[28] For Hesse, Uenze has argued against any connection between menhirs and *steinkammergräber*.[29] Uenze supports Kirchner's view that in Middle Europe menhirs were erected as the result of an independently moving idea. Presumably, however, the various communities who put up the stones must have already held certain magical concepts in common which would have been amenable to expression in this new mode. It is therefore not altogether unreasonable to look for some common cultural and economic background for these communities, and in this connection Kirchner's map of menhirs in Middle Europe should be compared with Irwin Scollar's recently published map of the provinces of the Michelsberg Culture.[30]

In conclusion it may be said that the relative sequence in Central Germany of the various monuments discussed would appear to have been: first, the menhirs with a general western derivation not necessarily dependent on megalithic tombs. Second, two things: (*a*) the animal art in the tomb at Lohne (Züschen), apparently of southern origin with affinities in the Maritime Alps; and (*b*) the house-decoration art of the Halle graves, found also at Lohne as a final and intrusive embellishment. This art is an eastern characteristic in the Single Grave Culture. Finally should have come the *statue-menhir*, a form of iconography again at home in the coastlands of the Ligurian Sea, related to, but not integral with, the art of the western Passage Graves, and possibly of somewhat greater antiquity.

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[28] S. J. De Laet, *The Low Countries* (1958), 110.

[29] Uenze (1956), 102–6.

[30] *Proc. Preh. Soc.*, XXV (1959), pl. IX, opposite p. 120. Equally applicable to the Belgian menhir distribution v.: M. E. Mariën, *Oud Belgie* (1952), fig. 125.

Kartengrundlage: Topographischer Atlas von Bayern, erste Auflage (1812 ff.)

Scale
0 0,5

Red : Bank of the Oppidum. Excavations 1955 - 58.

: 50 000

2 3km

The *Oppidum* of Manching

by WERNER KRÄMER

Dr Krämer, Director of the Römisch-Germanische Kommission des Deutschen Archäologischen Instituts, describes the excavations he has been carrying out for the last few years in the great oppidum *of Manching, near Ingolstadt, in Upper Bavaria. The article was translated by Professor Dr Bersu and Dr I. Scollar.*

ONLY a few decades after the conquest of Gaul by Caesar the power of the free Celtic tribes in central Europe collapsed as a consequence of their finding themselves placed, during the course of the 1st century B.C., in an insecure position between the Romans and the Germans pressing down from the North. The victorious Alpine campaign of Drusus and Tiberius in 15 B.C. sealed the fate of, among others, the Vindelicians who occupied the south German area north of the Alps as far as the Danube. Here, still today, mighty hillforts bear witness to the power of those nameless Celtic chieftains who caused them to be erected. Contemporary literary sources tell all too little about the history of this area and about the cultural connections of its inhabitants before the Roman occupation. Therefore modern research relied upon Caesar's description of the Gallic tribes in drawing parallels between the large late La Tène hillforts in central Europe and the city-like tribal centres of the Gauls in France, which Caesar called ' oppida ' or even ' urbes '.[1]

There were three reasons why no systematic excavations had up to now been undertaken in the areas inclosed by the banks of these south-German *Oppida*: (1) The enormous dimensions of these sites; (2) the often inaccessible siting of the forts in the forests; (3) test-excavations in some of them produced very few finds and no remains easily traceable as there are no buildings in stone.

No wonder, therefore, the question was raised whether one had to deal on all these sites on German soil with *oppida* of the Gaulish type.[2] It was suggested that most of our big walled enclosures, many of which exceed in size the classical *oppida* of France and still more the British sites, were only *refugia* in times of trouble and that no ' urban life ' flourished inside them.

The construction of a military airfield in the famous *Oppidum* of Manching gave impetus to the first extensive excavations at this important site.[3] The map (FIG. 1) dating from 1810 shows the situation and the outline of the bank in the middle of the quite flat, broad Danube valley south of the river. Its western limit is formed by the tributary Paar. It

[1] P. Reinecke, ' Spätkeltische Oppida im rechtsrheinischen Bayern ', *Der Bayerische Vorgeschichtsfreund*, 9, 1930, pp. 29–52. J. Werner, ' Die Bedeutung des Städtewesens für die Kulturentwicklung des frühen Keltentums ', *Die Welt als Geschichte*, 5, 1939, 380–90.

[2] K. Bittel, ' Das keltische Oppidum bei Finsterlohr ', *Jahrbuch Württembergisch-Franken*, New Series 24/25, 1949/50, 69–86.

[3] For the most important literature see W. Krämer, ' Zu den Ausgrabungen in dem keltischen Oppidum von Manching 1955 ', *Germania*, 35, 1957, pp. 32–45, and ' Manching, ein vindelikisches Oppidum an der Donau ', *Neue Ausgrabungen in Deutschland* (Berlin, 1958), 175–202.

looks as if, when the *vallum* was built, in order to make the necessary space for the *Oppidum* the Paar was diverted to the north as also was a small brook coming from the south. So the *Oppidum* is well placed above the level of inundation on a terrace, and marshy depressions to the north and south with the Paar in the west offer natural protection. Thus Manching is situated like the Gaulish *Oppidum Avaricum* 'prope ex omnibus partibus flumine et palude circumdata' (Caesar, *De Bell. Gall.*, VII, 15).

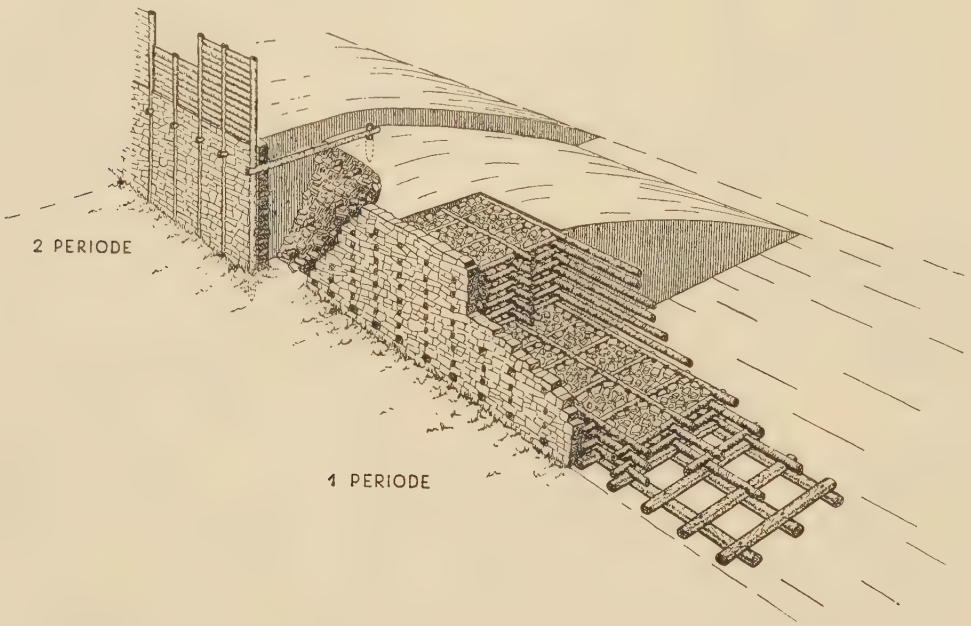


Fig. 2. Reconstruction of the two phases of the bank (after K. H. Wagner).

The size of our *Oppidum* with its bank extending originally over 7 km. in length is impressively demonstrated by a comparison with the 18th-century-fortification of the nearby fortress-town of Ingolstadt. (At the time of our map (FIG. 1) Ingolstadt had 6,300 inhabitants.)

The deciding factor for the choice of the place was its position in relation to traffic routes: From early times, the Ingolstadt region played an important role in the traffic from south to north across the Danube, while the narrow terraces on the south bank of the river provided an east-west communication right through the middle of the Manching *Oppidum*. This east-west route developed as an important highway in Roman times. Several early- to middle-La Tène flat grave cemeteries, of which the best known on the so-called ' Steinbichel '[4] lies outside the enclosure in the modern village of Manching, imply settlement of the southern bank of the Danube before late La Tène times.

Until the construction of the airfield in 1936 completely altered the landscape, the interior of the enclosure was occupied for the most part by fields and copses belonging to the village of Manching. A systematic excavation was, at that time, only possible on the bank itself which had to be removed mainly on the eastern side of the *Oppidum*. Here,

[4] Reinecke, *Altertümer unserer heidnischen Vorzeit*, vol. 5, pl. 51.

in 1938, K. H. Wagner[5] made the well-known observation that the outer face of the bank had dry walling in stone and wood and had been constructed in two phases (FIG. 2): the older was a true *murus Gallicus*.[6] In the later phase a refacing took place which, following an older prehistoric technique, was built with vertical wooden posts visible from the outside. Up to now, Manching is the easternmost representative of the *murus Gallicus*.[7] Of course, many of the great hillforts of south Germany and Czechoslovakia have never been examined with regard to the construction of their banks. It would be particularly helpful to know the type of the banks of the big *Oppidum* near Kelheim, 30 km. downstream from Manching which lies on the northern bank of the Danube at its junction with the Altmühl.[8]

In 1955, as the war-damaged airfield was again to be made operational, the ' Bayerische Landesamt für Denkmalpflege ' started excavations with the primary aim of answering the question whether and how the 900 acres enclosed area of the *Oppidium* was settled. The ground was therefore examined by numerous trial-trenches, cut with a mechanical excavator, over 7·5 km. in length (map, FIG. 1). The results were a surprise to us. We found in the midst of the *Oppidum* definite indications of intensive Celtic occupation extending at least 1·5 km. from east to west. Only a strip a few hundred metres wide along the inside of the bank appeared to have remained free of buildings. It may have served equally well for manoeuvring by soldiers who had to defend the wall as also for the containment of large herds of animals.

After this ' trial excavation ', we began to uncover an area in the middle of the *Oppidum* in order to get information about the character of the settlement. We found the ground-plans of houses (PLATE XXI (*a*)) and of long structures with straight walls built by vertical wooden posts sometimes set in bedding-trenches. Stone was not used for the buildings. One may even discern a systematic scheme in the arrangement of the buildings with an approximate east-west and north-south orientation.

Although the excavated area is not a small one, none of the uncovered building complexes can so far be defined completely nor can their functions as yet be unambiguouslyexplained. It should be remembered that our excavation is only a beginning. The 14,200 square metres uncovered up to now are barely 0·5 per cent of the total area inside the *Oppidum*.

Apart from the post-supported structures a large number of round pits were found, dug vertically into the ground. I believe they can only be interpreted as storage pits. This view is supported by remains of quadratic wooden lining (PLATE XXI (*b*)) or of round (barrel-shaped ?) wooden containers, often met by us in the pits. These pits were frequently renewed. Obsolete older pits were in part filled in with the excavated earth from new ones and with rubbish of all kinds.

The excavated area of 1957 (FIG. 3) shows that 160 such pits were found, whose arrangement, despite much overlapping, allows one to recognize a distinct plan. The obvious rectangular corner in the arrangement may mark out a boundary of a property. Its orientation corresponds to that of the structures lying farther to the south. I have compared these pit-complexes already with the storerooms of palaces and towns of classical times, where earthern storage vessels (*dolia* or *pithoi*) have exactly the same functions and are arranged similarly to our groups of wood-lined pits.

[5] K. H. Wagner, ' Untersuchungen am Wall der spätkeltischen Stadt von Manching, Landkreis Ingolstadt ', *Bayerische Vorgeschichtsblätter*, 16, 1942, 10–20.

[6] For *murus Gallicus* see W. Dehn, *Germania*, 38, 1960, 43–55.

[7] Cf. M. Aylwin Cotton's map in R. E. M. Wheeler and K. M. Richardson, *Hill-Forts in Northern-France* (London, 1957), 165, fig. 35, and K. Bittel, *loc. cit.*

[8] The *oppidum* of Kelheim: Reinecke, 24/25. *Bericht der Römisch-Germanischen Kommission*, 1934/35 (1937), 167 f.

Fig. 1.

THE FINDS

The enormous number of finds made it indispensable to interrupt field work in 1956 and 1959, in order to have time to work them up. The further planning of the excavation will essentially depend upon how the work on the finds progresses and what information

they yield. As I have previously pointed out, our earliest finds correspond to the material from the flat graves of the middle La Tène period (Reinecke's phase C) and it looks as if the settlement continued on without interruption into the late La Tène period (Reinecke D). But we have to state that this relative La Tène chronology in the zone north of the Alps has not yet been satisfactorily defined with respect to its type-inventory. Further, the work on the finds from Manching has not yet progressed enough to draw exact chronological conclusions concerning the beginning of settlement of the *Oppidum*.

Nevertheless, there are already quite interesting results about the economic history. Of course, it is very difficult to calculate even approximately the number of inhabitants of the *Oppidum*. The size of the inhabited area and the great labour-force needed for building and rebuilding the bank warn against too small an estimate. Products of specialized local handicraft strongly suggest that the ' townsmen ' could not have made a living from the yields of their own agriculture alone. They were compelled to draw on the food production from outside and, as the many pits show, to store food in considerable quantities.

In the *Oppidum* they kept great herds of animals in the uninhabited strip behind the bank. Further, pigs may have found much of their food in the extensive marshy woods which were easily accessible from the interior of the *Oppidum* via the large southern gate. The animal bones found up to now—with more than a quarter of a million finds, the largest body of systematically examined material of this sort in Germany—have been determined by Dr J. Boessneck and his collaborators at the Institute for Animal Anatomy of the University of Munich. I owe the following data to him: More than 99 per cent of the bones come from domestic mammals among which cattle bones with 40·5 per cent predominate. Then come pig bones with 34·9 per cent, sheep or goat with 18·8 per cent, horse with 4·8 per cent and the dog with 0·9 per cent. But the percentage of the various kinds of animals gathered in this way must be corrected when the number of single individuals to which these bones belong is to be ascertained. This has been done, up to now, only for the finds of 1955: At least 3,100 domestic animals of various kinds were counted of which the pig, with 1,350 individuals, is the most frequently represented. Sheep and goats follow with 800 specimens, cattle with 700, the dog with 139 and the horse with 77. The larger wild animals occur very rarely. This may be a hint at the size of the population of the *Oppidum* whose inhabitants almost exterminated the game in the nearby forests.

Next to the animal bones, potsherds are the most numerous finds, among which wheel-turned pots strongly predominate.[9] The comb-ornamented pottery ('Kammstrich-Keramik ') made of clay, strongly mixed with graphite which made them fire-resistant, served for cooking (PLATE XXII (*a*)). Some of these pots have maker's marks which were scratched on their bases before firing. This indicates a production of this ware in potteries on an industrial scale. Finds of bone tools with serrated edges which were used to make the combed ornament show that this pottery was also made at Manching. But kilns have not been found as yet. They may well lie in another part of the *Oppidum*, if not *extra muros* as other industries which might give rise to fire hazards. The mass of the undecorated wheel-made ware served for storing food and drink or as table ware (PLATE XXII (*b*)). The well-known painted pottery, so characteristic of the late La Tène period, may have been used for keeping distinct varieties of liquids (PLATE XXII (*c*)).

The need for iron must have been considerable. The enormous number of heavy iron nails used in the construction of the *murus Gallicus* and, on the other hand, the number of iron objects which turned up in the excavation show this clearly. Smithies lay in the excavated area as proved by finds of small bowl-shaped slag lumps (PLATE XXIII (*b*)). But

[9] Up to now about 110,000 sherds were found and from these 335 pots could be reconstructed.

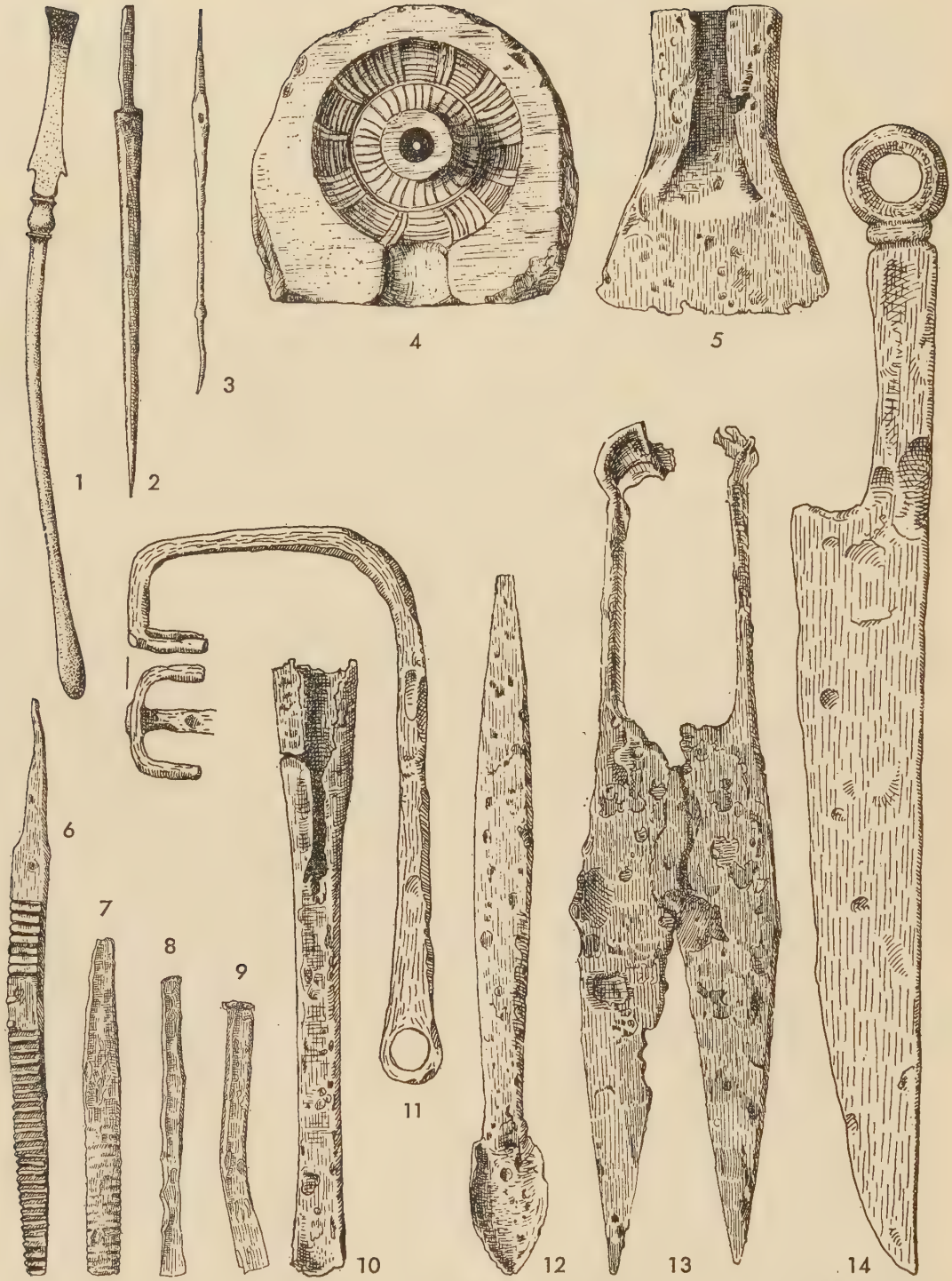


Fig. 4. Tools and utensils (1, bronze; 2–3, 5–14, iron) and stone mould (4). Scale 1, 4 = $\frac{2}{3}$. 2, 3, 5–14 = $\frac{1}{2}$.

smelting of iron took place *extra muros* in the marshy land south and east of the *Oppidum*, where there is an ample supply of easily-collected bog iron-ore. Years ago heaps of large lumps of slag were found in that area during agricultural operations.

There were also copper and bronze craftsmen who left fragments of their furnaces (PLATE XXIII (*a*)), tools (FIG. 4) like punches and files, and a stone casting mould (FIG. 4 (4)) as witness of their activity. It is likely that these craftsmen worked not only for the domestic needs, but also traded their products. That the home consumption may have been very large is indicated, to quote but one example, by more than 300 fibulae (FIG. 5) found in our excavation. Half-finished pieces prove that some of them at least were fabricated in the *Oppidum*. Good examples of late La Tène bronze-industry are mountings of horse-collars and trappings decorated in Celtic style (FIG. 6 (1–3)).

Our excavation enables us to make an important contribution to the early history of glass making. Reinecke maintained the opinion that the well-known middle and late La Tène glass bracelets and glass beads were made in workshops of the classical lands of the Mediterranean for export to the barbarians. A big chunk of raw glass, purple in colour, found in 1957 (PLATE XXIV (*b*)) proves the contrary: moreover this indicates that bracelets and beads of identical purple colour which occur so frequently in Manching were made there. (The question of where the raw glass itself was made remains open.) Altogether we found 146 fragments of bracelets of various colours and 70 annular beads.

The importance of the Manching *Oppidum* as a centre of the Vindelicians is stressed by the existence of a mint there. So we can assume that the mint's owner, a Vindelic chieftain, had his residence here. More than one hundred fragments of the characteristic moulds made of clay (' Tüpfelplatten ') in which the metal for the coin-flans was smelted (PLATE XXIV (*a*) (1–5)), prove the existence of the mint. Analogies are known from *oppida* in England, France and Czechoslovakia.[10] The Manching moulds, differing in form and size from each other, were used for various types of coins. In some such moulds, drops of metal are still preserved, and analysis made by Professor Specht (Munich) showed that they are of the same alloy as that of several barbaric gold coins known as ' Vindelician Regenbogenschüsselchen ' which turned up in our excavation (PLATE XXIV (*a*) (6–9)). In the last century two hoards of such gold coins were found near Manching. A further hoard of ' north Helvetic ' silver coins was found in the *Oppidum* when the airfield was built in 1936.

Fragments of Italian amphorae, which certainly once held the wine so highly esteemed by the Celts, testify to a far-reaching commerce which found its way over the Alpine passes. A few fragments of ' campanian ' pottery and coloured *millefiori* glass as well as fragments of north Italian bronze vessels (FIG. 6 (4–6)) connected with the luxury of wine-drinking.

The Celts on the Danube also maintained connections with the barbarian tribes in the southern Alpine valleys. Such is shown by women's jewellery, as three large bronze *fibulae* (FIG. 5 (8)) which occur also in the cemeteries of Tessin and in the south Tirol, where the archaic early Le Tène form was still fashionable in the middle and even in the late La Tène period. This jewellery was more probably not brought over the Alps to Manching by a trader but by its wearer. The same applies for three large bronze *fibulae* with a large disc (FIG. 5 (9)) which came from the opposite direction, namely from the northern German area.[11] We know that marriages happened with members of far-distant barbarian tribes

[10] Cf. K. Castelin, ' Keltische Münzformen aus Böhmen ', *Germania*, 38, 1960, 32–42.

[11] Krämer, ' Ein außergewöhnlicher Latènefund aus dem Oppidum von Manching ', *Reinecke-Festschrift* (Mainz, 1950), 84–95.

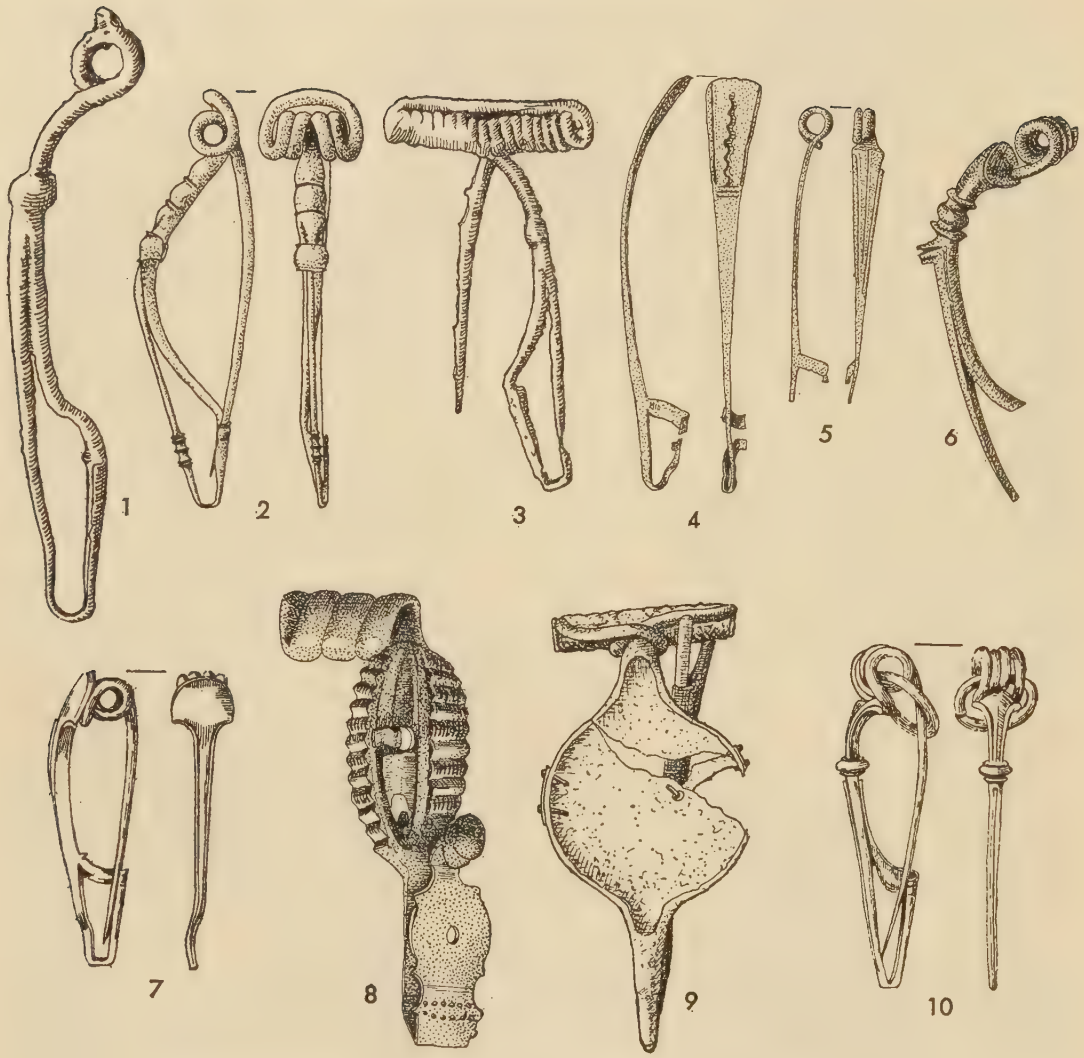


Fig. 5. Brooches (1, 3, iron; 2, 4–6, 8–9, bronze; 7,10, silver). Scale $\frac{2}{3}$.

in the last century before the birth of Christ from Caesar's report of the second marriage of Ariovistus with a Celtic princess from Noricum (*De Bell. Gall.* I, 53).

There is ample evidence of the violent fate the *Oppidum* of Manching suffered when the Romans occupied the country of the Vindelici in 15 B.C. All over the settlement swords, spearheads, and iron shield bosses are scattered, most of which have been broken deliberately and rendered useless (FIG. 7). Altogether more than 100 swords and 40 spearheads or fragments of these weapons have been found up to now. It appears to me most unlikely that Manching continued to be inhabited after the conquest of 15 B.C., for Roman imports and other objects of the 1st century A.D. are utterly lacking.

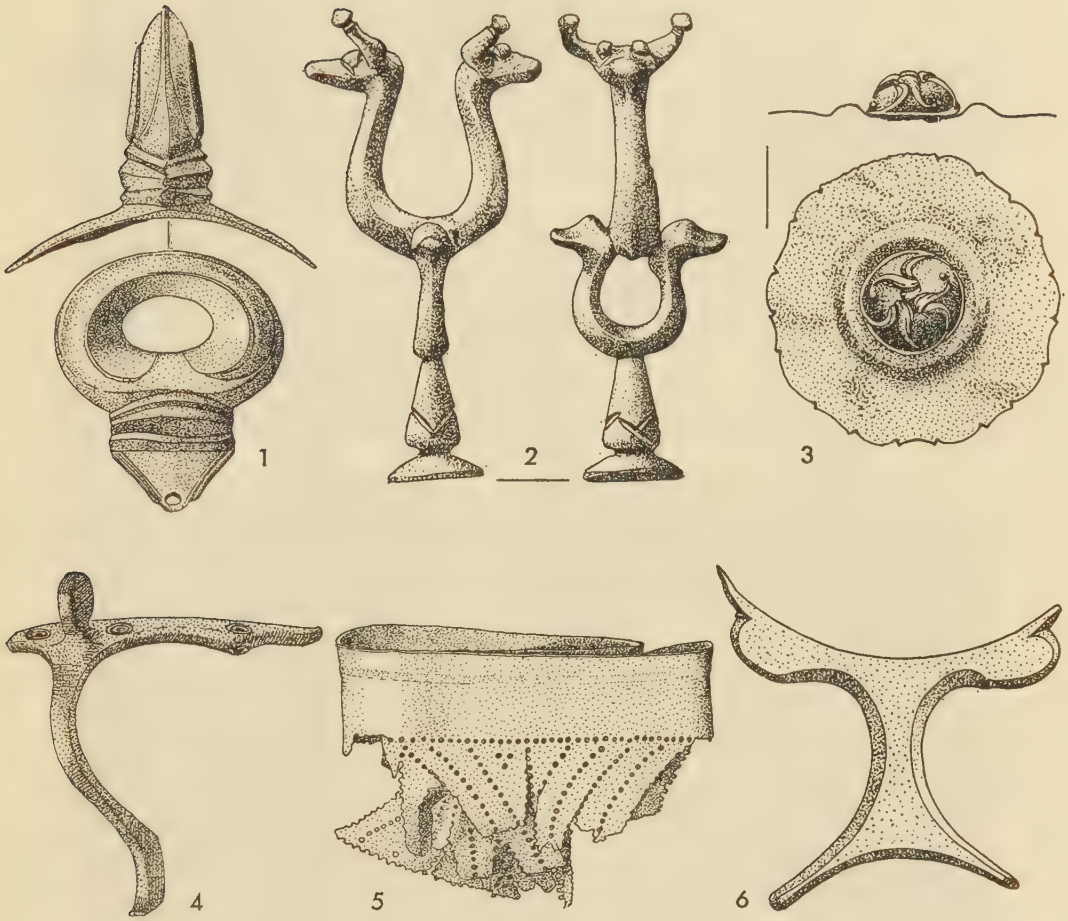


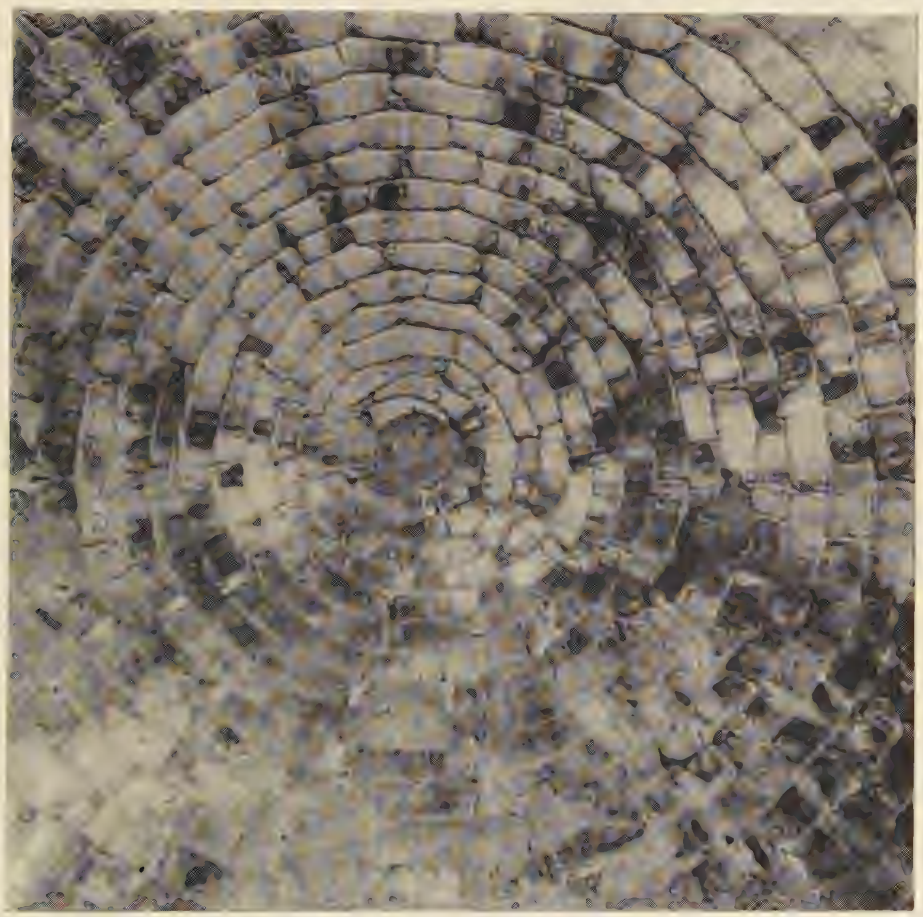
Fig. 6. Native (1–3) and Roman (4–6) bronze work. Scale $\frac{2}{3}$.

After the destruction, the place must have been quite a long time deserted until a small Roman '*mansio*', situated on the south Danube road which cuts right through the old *Oppidum*, was established. Further, in the late Empire, a Roman fort, named 'Vallatum' is supposed to have been situated in the old *Oppidum* and to have taken its name from the big banks.[12] (Fig. 7, see p. 200)

[12] Reinecke, 'Zur Geschichte und Topographie von Vallatum', *Sammelblatt des Historischen Vereins Ingolstadt*, 59, 1950, 3–38.

Fig. 7. Manching. Weapons and military equipment (1, bronze; 2–7, iron). 1. Cheek-piece of a helmet. 2. Chain from sword-belt. 3. Scabbard. 4–5. Swords. 6. Spearhead. 7. Shieldboss. Scale: 1–3 = ½. 4–7 = ¼.

(*a*)



(*b*)

THOLOS TOMBS OF THE AEGEAN

(*a*) *Early circular tomb at Platanos* (*Tholos B*) *in the Mesara.* (*b*) *Treasury of Atreus at Mycenae: looking up at the vault from the floor.*

See p. 166]

(*a*) *Remains of a little Late Bronze Age* tholos *tomb at Vourvoura, between Sparta and Arcadia.*

(*b*) *Early circular tomb of the Middle Minoan period at Knossos. In the foreground, remains of the circular chamber, with the blocking slab in place in the entrance at the level of the third and last floor. The half metre measure is lying on the original floor, with burials in position on the second floor above it. In the background a rectangular ossuary filled with bones from burials removed out of the circular chamber.*

See p. 166]

(*a*) *Wall of the chamber in the Kephala* tholos *tomb at Knossos.*

(*b*) *Entrance of the second* tholos *tomb at Dimini in Thessaly.*

THOLOS TOMBS OF THE AEGEAN

See p. 166]

(*a*)

(*b*)

MEGALITHIC AND OTHER ART

(*a*) *Dölauer Heide, stone 9.* (*b*) *Dölauer Heide, stone 5.*

See p. 180]

[*Photos :* Landesmuseum Halle/Saale.

(a)

(b)

MANCHING

Excavations 1955: (a) Detail of a house (postholes and bedding-trench). (b) Quadratic lining at the bottom of a storage pit.

See p. 191]

(*a*)

(*b*)

(*c*)

MANCHING

Pottery from the oppidum: (*a*) *Comb-ornamented graphite ware.* (*b*) *Undecorated wheel-made ware.* (*c*) *Painted late La Tène ware.*

See p. 191]

(*a*)

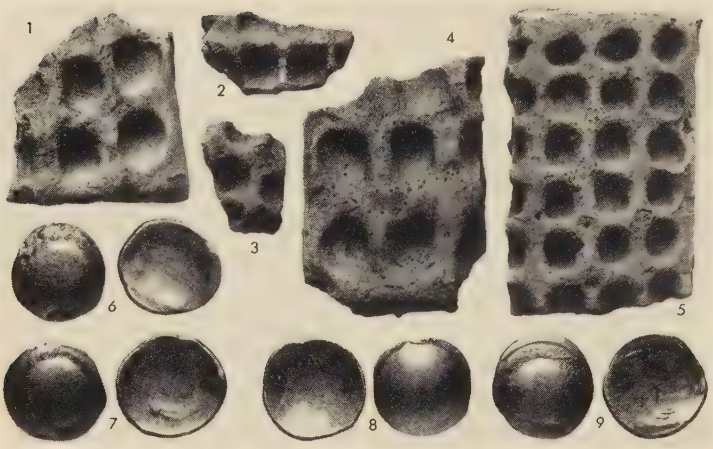
(*b*)

MANCHING

(*a*) *Nozzles of furnaces for smelting copper.* (*b*) *Objects from a smithy: heap of slags, hammer, tongs.*

See p. 191]

(*a*)



(*a*) *1–5 Coin-mould fragments. Scale* $\frac{1}{3}$. *6–9 Gold coins* (' *Regenbogenschüsselchen* '). *Scale* $\frac{2}{3}$.

(*b*)

MANCHING

(*b*) *Chunks of purple glass, fragments of bracelets of same colour, purple and yellow annular beads. Scale about* $\frac{2}{3}$.

See p. 191]

THE CONSTANTINIAN BASILICA OF THE LATERAN

Constantinian foundation walls of the Lateran Basilica. Walls C (right) and D intersecting the vaulted cellars of the third-century barracks

See p. 201]

A CELTIC MIRROR FROM GREAT CHESTERFORD

The glossy face of the mirror. Overall length 23·5 cm (11·75 in) and diameter of the plate 16·3 cm (8·15 in).

See p. 207]

[*Photo:* Museum of Archaeology and Ethnology, Cambridge.

A CELTIC MIRROR FROM GREAT CHESTERFORD

The mirror-back. The dark strip (bottom left) is partly a crack and partly a shadow cast by lateral displacement.

See p. 207]

[*Photo:* Museum of Archaeology and Ethnology, Cambridge.

(*a*)

(*b*)

(*c*)

(*d*)

A CELTIC MIRROR FROM GREAT CHESTERFORD

Details of the pattern on the mirror-back; (a) Left, at '8 o'clock' (b) Right, at '2–3 o'clock', (c) Left centre, at '10 o'clock', (d) Right centre, at '4 o'clock'.

See p. 207]

[*Photos:* Museum of Archaeology and Ethnology, Cambridge.

The Constantinian Basilica of the Lateran

by RICHARD KRAUTHEIMER AND SPENCER CORBETT

Richard Krautheimer is Professor of the History of Art in the Institute of Fine Arts of New York University and Director of the publication Corpus Basilicarum Christianarum Romae. *Spencer Corbett, formerly Librarian at the British School at Rome, is now Research Associate of New York University and is Professor Krautheimer's collaborator in the preparation of the* Corpus.

RECENT investigations at the Lateran by the present writers, with the indispensable collaboration of Professor Enrico Josi, and under the auspices of the Pontifical Institute of Christian Archaeology, have added considerably to our knowledge of the Constantinian basilica. The 4th-century foundation walls of this church are easily identifiable. They consist of massive baulks of concrete made principally with big fragments of grey-white marble which are obviously the product of the breaking-up of some quite important building, since pieces of architectural mouldings, capitals, ashlar, etc., frequently appear. During the past century (the first significant excavations date from 1851) such foundation walls have been exposed in many places, and an extensive system of vaults has been formed beneath the floor of the church as one investigation after another has added to our knowledge of the Constantinian building; to say nothing of the earlier structures on top of which the basilica was built, namely the *Castra Nova Equitum Singularium* of the early 3rd century, and certain rich private mansions which lie at a still lower level.[1]

Prior to the studies which are briefly summarized in this article,[2] the following parts of the Constantinian church were known: (*a*) the main apse foundations; (*b*) part of the foundation of the end-wall of the inner south aisle, close to the left shoulder of the apse; (*c*) the foundations of both main nave colonnades from points close to their junction with the apse, for about one-eighth of the total length of the nave; (*d*) two more short lengths of the main nave colonnade foundations on the right hand (north) side of the nave.

A new examination of a cellar which lies beneath the pavement of Galilei's portico has now revealed four blocks of the characteristic marble foundation material projecting into the cellar from its west side. Evidently these are the eastern extremities of the foundation walls of the four colonnades which divided the original basilica into a central nave and four side-aisles. Their breadth corresponds with the known thickness of the nave foundation walls. Their spacing corresponds with that of the present aisles and shows, what was hitherto uncertain, that the 17th-century aisles have exactly the same breadth as the Constantinian ones, all four of Borromini's rows of pillars standing in the alignment of the original colonnades and using the original foundations. The extension of the original

[1] The fullest account of all excavations at the Lateran up to 1944 is given by A. M. Colini in 'Storia e Topografia del Celio nell'Antichita' (*Memorie della Pontificia Accademia Romana d'Archeologia*, vol. VII), 343–77.

[2] Detailed accounts of these studies have appeared in *Rivista di Archeologia Cristiana*, vol. XXXIII (1957) 79–98, and vol. XXXIV (1958), 59–72.

foundations eastwards to a point which is nearly 5 m. outside the east façade has yet to be explained. It may be that Constantine's basilica was longer than the mediaeval one[3] or, alternatively, that its foundations continued underground beyond the line of the façade for some structural reason which escapes us. As yet no trace of the original façade foundations has appeared.

A sounding in the area beneath the south transept has produced important results (FIG. 1). The transept happens to lie on top of a vaulted 3rd-century building which fortunately enables us to approach the church substructures without the inconvenience of raising the floor. The 3rd-century building consists of two parallel underground galleries, roofed with concrete cross vaults, which are undoubtedly the cellars of a long barrack building belonging to the *Castra Nova*. The portion of these cellars which lies beneath the central zone of the transept has long been known, and so too has the extension of the same cellars which lies under the 12th-century cloisters to the south, but the intervening portion under the south part of the transept has not. When the Constantinian foundation wall of the south nave colonnade (A on the plan) was pierced during the excavations of 1934–8,[4] the vaults to the south were found to be full of rubble and no further excavations were then undertaken. This rubble has now been partly removed, enabling us to enter the 3rd-century cellars beneath the south part of the transept and examine the Constantinian foundations which intersect them.

At a distance of 7 m. from the south side of wall A the 3rd-century gallery is intersected by another foundation wall (B), also apparently Constantinian, though constructed in masonry rather different from the usual concrete of marble fragments. Although B is not quite parallel with A it stands nearly enough in the alignment of the outer south colonnade. Whereas the main nave foundations (A) pass through the barracks vaulting, which was cut away for them, wall B merely impinges on the underside of the vault. We presume that the stylobate above rests on the extrados. The difference between walls A and B reflects the fact that the main nave colonnade bore a much greater weight than the outer one. The slight obliquity of wall B is no more than a builders' mistake; as the pre-existing vault was not pierced they were less able to check the wall's straightness than was the case with A.

Continuing our investigations, a hole was cut through wall B (its thickness, 1·70 m., is the same as that of wall A) enabling us to enter the part of the 3rd-century cellar which lies beneath the south end of the transept. Here we were surprised to find a typical Constantinian foundation wall built with the usual marble fragments (C on the plan) lying parallel with the axis of the cellars and thus at right-angles to the two foundation walls which we have traversed. After proceeding in a north-south direction for nearly 11 m. the masonry forms a right-angle and becomes wall D (PLATE XXV). Wall D lies directly underneath the south wall of the transept. The southern, or external face of wall D, and the angle between C and D is also seen in the southward continuation of the cellars, underneath the cloisters.[5]

Wall D crosses the western gallery of the cellar and continues for the full breadth of the parallel eastern gallery. Walls B, C and D thus form three sides of an approximately rectangular compartment, and in order to establish the eastern limit of the compartment

[3] The medieval east wall of the basilica is still intact, though it is hidden by Borromini's and Galilei's additions.

[4] E. Josi, ' Scoperte nella basilica costantiniana al Laterano ', *Rivista di Archeologia Cristiana*, vol. XI (1934), 335 ff.

[5] *RAC* (1957), cit. 84–94.

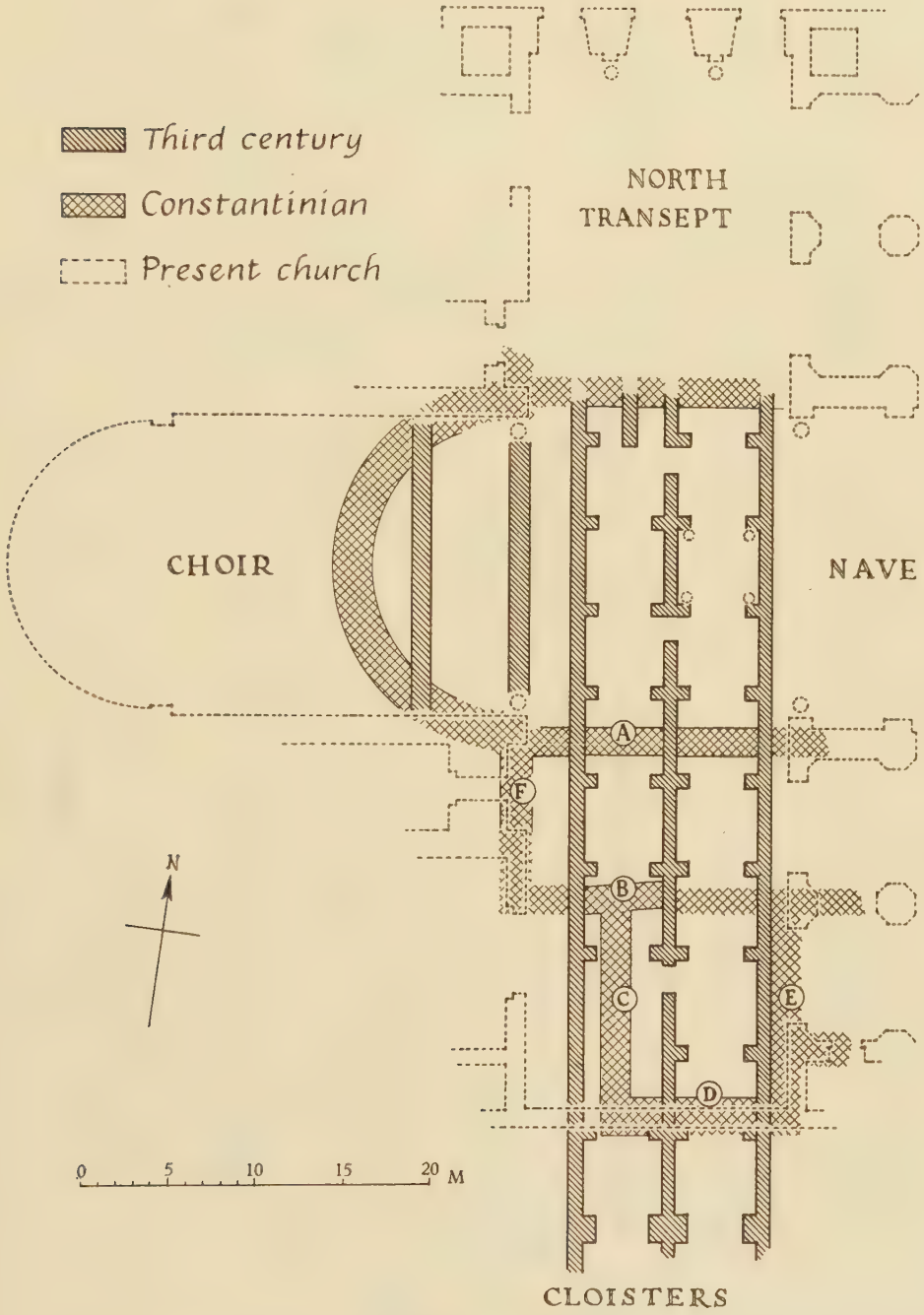


Fig. 1. Lateran Basilica. Plan of the structures underlying the transept.

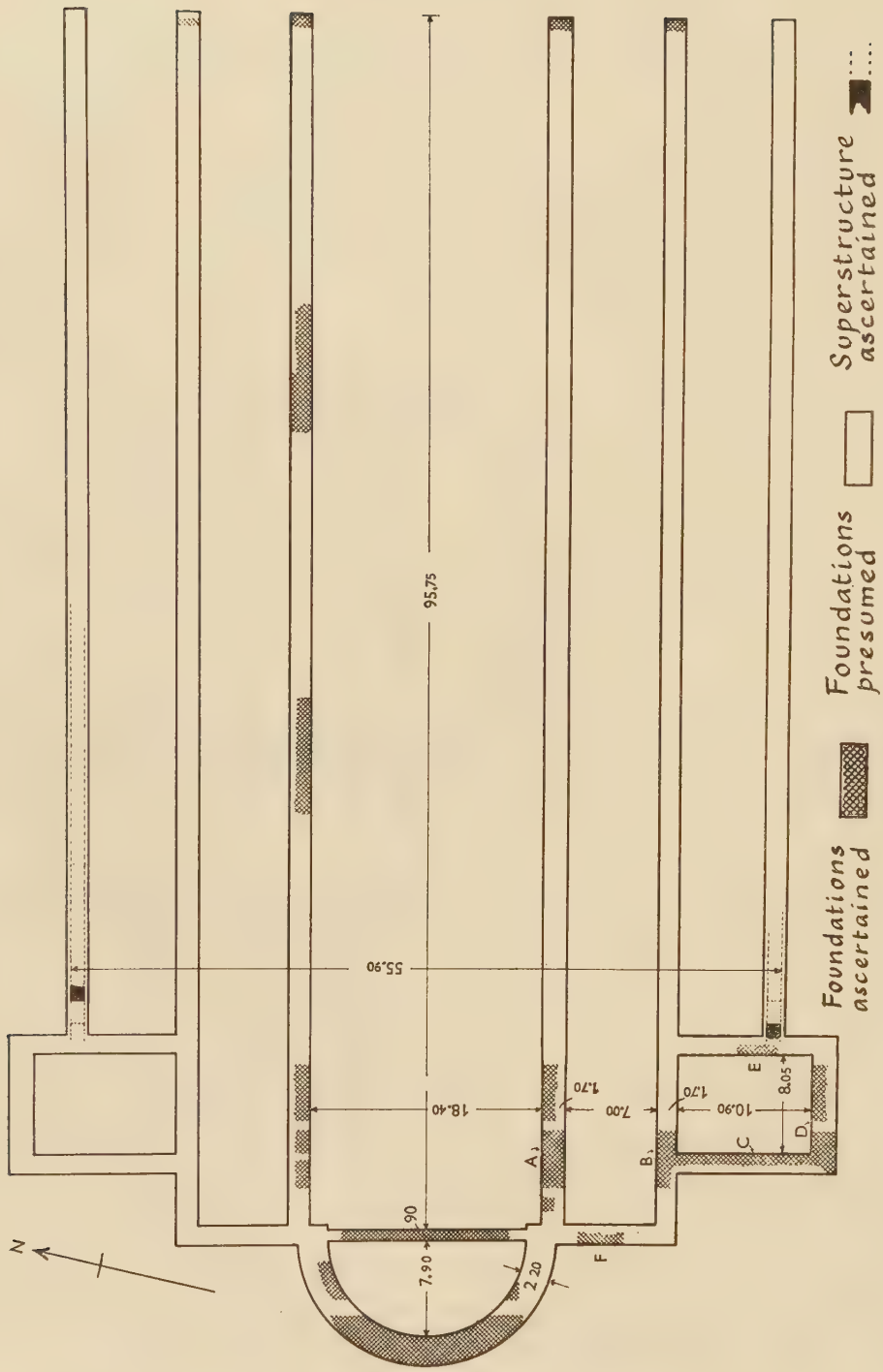


Fig. 2. The 4th-century Lateran Basilica. General plan of ascertained elements.

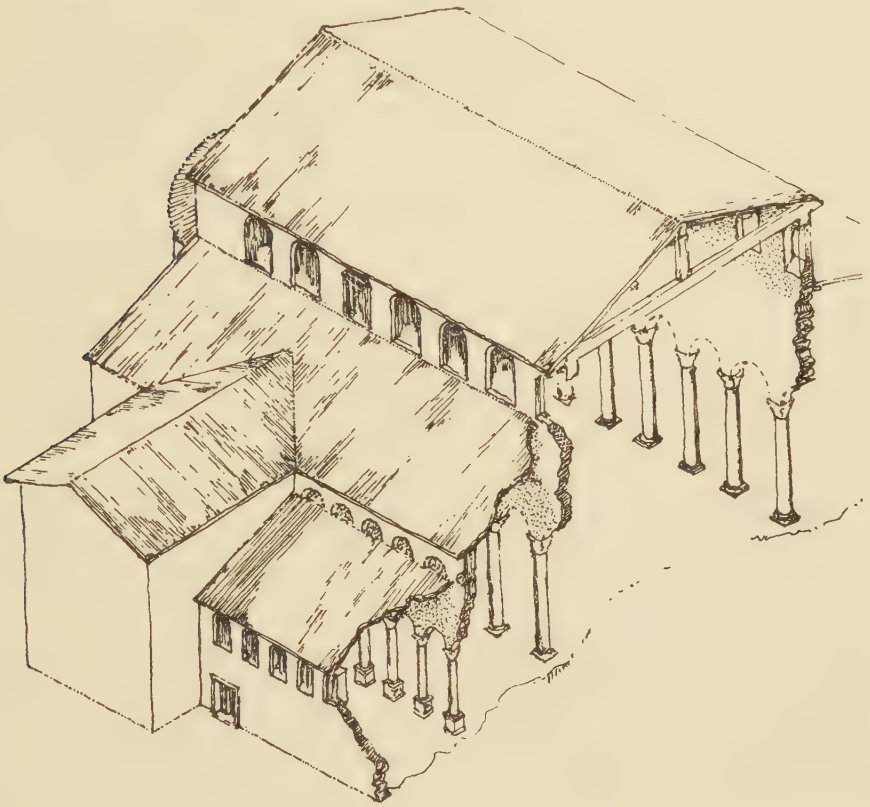


Fig. 3. Tentative sketch reconstruction of the western part of the original basilica.

we made two small openings in the east side wall of the gallery. The fourth wall immediately appeared (E on the plan) touching the exterior of the barrack building.

So much for the foundations. Of the superstructure of the Constantinian church very little has yet been found. Certainly the original colonnades and clerestory walls of the main nave have perished; everything that can be seen behind and above the 17th-century decorations is evidently of late medieval date. But the side-walls of the outer aisles, where they have not been removed to make entrances to successive side chapels, seem here and there to include fragments which survive from the Constantinian structure. Two such 4th-century fragments are especially important because they contain the remains of windows, one on each side of the basilica, both near the point where the basilica walls are interrupted by the lateral projections of the transept. We surmise that there was a row of windows in each side wall, spaced to correspond with the arcading of the outer colonnades. The northern fragment also discloses a side entrance to the north aisle close to the angle now formed by the transept.[6]

The main outlines of the plan of Constantine's basilica can thus be reconstructed with some confidence (FIG. 2). In its western part the original church is now seen to differ fundamentally from all the hypothetical plans that have hitherto been advanced. It is

[6] *Ibid.*, 79–84.

certain that there was no transept in the usual sense of the word; and it may be noted in passing that the masonry of the present structure (wherever it can be seen) is all of late medieval date. Neither was it a simple basilica of four aisles continuing for the whole length of the nave and terminating against an end-wall in line with the apse front.[7] Nor, for that matter, was there a tri-partite or a quin-partite transept[8] as was once proposed in explanation of the continuous nave stylobates beneath the transept. We now know that the original church had the following features. The stylobates of the main nave colonnades continued, apparently uninterrupted by a Triumphal Arch, right up to the apse front. The inner aisles had the same length as the main colonnades; witness the foundations of the end-wall (F) on the south side of the apse. The outer aisles, on the other hand, were some 15 m. shorter than the inner ones, ending at wall E. To the west of wall E there was a projecting, wing-like chamber which extended some 4 m. beyond the alignment of the aisle wall. Its increased breadth suggests that the roof of the wing chamber was higher than the aisle roof. It seems likely to have appeared on the outside of the basilica as a lateral annexe, having some of the characteristics of a transept, but unconnected with the nave. It might perhaps be termed an aisle-transept (FIG. 3).

Inside the church the means of communication between the aisle-transept and the adjacent aisles must remain an open question. Our ignorance of what stood on top of the foundation walls allows several hypothetical combinations of archways, doors, solid walls and colonnades. Likewise, the function of the chamber remains obscure; though it may seem not unreasonable to guess that it could have served for the reception of offerings at the beginning of the *missa fidelium*.

Our studies have been confined to the south half of the basilica and the existence of corresponding features on the north is, as yet, hypothetical. However, the massive scale of the foundations discovered proves that they supported buildings of monumental character and, as there is nothing to suggest that the Lateran Basilica was asymmetrical, we presume the existence of corresponding foundations underneath the pavement in the north half of the transept.

[7] J. B. Ward Perkins, ' Constantine and the Origins of the Christian Basilica ', *Papers of the British School at Rome*, vol. XXII (1954), 84 ff.

[8] R. Krautheimer, *Actes du v*^{eme} *Congrès International d'Archéologie Chrétienne* (1954), 286.

Book Chronicle—*continued from p. 176*

ARCHAEOLOGY IN CHINA. *By* WILLIAM WATSON. *pp.* 32, 132 *plates. London, Max Parrish*, 1960. 52*s.* 6*d.* A survey of some recent discoveries from the Palaeolithic Period to the end of the Han Dynasty based on 140 photographs from the exhibition of Chinese prehistory organized by the British-Chinese Friendship Association which recently toured Britain.

ARCHAEOLOGY IN THE HOLY LAND. *By* KATHLEEN KENYON. *pp.* 326, 66 *text figures*, 56 *plates. London, Ernest Benn*, 1960. 36*s.*

FROM CAESAR TO ARTHUR. *By* GEOFFREY ASHE. *pp.* 320, *London, Collins*, 1960. 21*s.* A study of the life and career of Arthur in so far as it can be traced through early documents and remains, by the author of King Arthur's Avalon (1957).

THE NEW-OLD LAND OF ISRAEL. *By* NORMAN BENTWICH. *pp.* 161, 13 *plates. London, George Allen and Unwin*, 18*s.* An account of the archaeology of the Bible Lands and of recent excavations there.

[*continued on p.* 210.

A Celtic Mirror from Great Chesterford

by SIR CYRIL FOX

A Celtic Bronze mirror was recently found, probably in the parish of Great Chesterford in North Essex. Dr Geoffrey Bushnell, Curator of the Museum of Archaeology and Ethnology in the University of Cambridge, invited Sir Cyril Fox to describe this fine new mirror for publication in ANTIQUITY, *and Sir Cyril wishes to acknowledge the help he has had in the preparation of this article from Miss M. D. Cra'ster, Assistant Curator in the Museum.*

THE mirror is likely to have come from a lady's grave: no details of the find are available. There is in the parish a well-known cemetery dating from the beginning of the Early Iron Age.

The overall length of the mirror with its handle (PLATE XXVI) is 23·5 cm. (11·75 in.); the diameter of the plate is 16·3 cm. (8·15 in.). This has a delicate incised line round its circumference, close to the edge, back and front, as the photographs show.

The face of the mirror is smooth and glossy: the piece suffered some damage when disclosed—probably by a plough-share. It is thus slightly bent at the top (losing, here, its smooth reflecting surface), and the handle has been wrenched, causing a crack in the plate to the right and above it. It should be noted that there is no loss of detail of the pattern on the mirror-back resulting from this crack: the dark strip on the photograph (PLATE XXVII) is, in part, a shadow cast by lateral displacement.

The elaborate pattern, to which we now turn, is a free-hand creation characteristic of Early Celtic craftsmanship: this is seen in the enlarged photographs of detail (PLATE XXVIII) where ' hair-lines ' drawn with a fine point, and marking the course the pattern is to take, sometimes survive beside the stronger, more assured, final outline of the work (PLATE XXVIII (*a*)). Miss Cra'ster noted, when making the drawing (FIG. 1) that ' all the curves are within the natural swing of the fingers, when holding a drawing-tool and pivoting on the side of the wrist '—a technical point that gives life to the structure!

The shading of the pattern is usually in blocks of three or four parallel lines, adjacent to others at right-angles (' cross-hatching '); but the curves of the pattern lead to minor variations of the technique. Plaiting, for example, can clearly be seen (PLATE XXVIII (*a*) and (*b*)) and simple shading (PLATE XXVIII (*d*)). The pattern as a whole is most easily grasped in the drawing (FIG. 1), which avoids the visual distractions due to corrosion.

The design is complex and markedly eccentric: two-thirds of the patterned area are on the left side of its vertical axis. A curve of wide sweep, at the very top of the plate, swings downwards and then inward on either side, nearly touching: then outwards,[1] and afterwards upward on either side, to terminate on the viewer's right in one node, and on his left in two. Two opposed nodal coils, on curved stems arising from the major units and linked by a hanging loop, fill the upper centre of the pattern: on the right there is a dual

[1] Its passage under the handle on the left is certain; it is visible in the notch marked X.

Fig. 1. The mirror, with its pattern outlines. Scale $\frac{2}{3}$.

internal frame ' stiffening ' the smaller coil; on the left a more complex frame (with a free node) to ' support ' the larger coil, with *its* two terminal nodes.[2]

TECHNIQUE

The detailed technique of the pattern—such as the varying breadth of the pattern-lines and the methods used in shading (giving mass to the structure), are illustrated (PLATE XXVIII (*a–d*)). The photograph (*a*), taken near 9 o'clock on the circle (PLATE XXVII) shows groups of incisions in the metal, meeting other groups either at right-angles or diagonally: the tradition of the mirror-makers (now illustrated by some fifteen examples), permitted wide freedoms. The original lay-out of our design was rapid free-hand work, using the sharp tip of a ' graver '; it was often ' improved ', as the outer and inner margins of the lower unit on the left of (*c*) show. Unit (*b*), featuring the node on the right side, ' at 3 o'clock ', shows the frequency of the plain cross-bar shading on narrow bands, and the casual character of much of the workmanship; it does not ' stand up to ' enlargement.

Another aspect of PLATE XXVIII (*a*) is worth mention. The node on the left of the design (at ' 9 o'clock ') is too close to the upper curve of the pattern below to be agreeable—it should surely be higher up ? I held this view for some time; but when examining the voids of the pattern, always significant in Celtic ornament, I realized that its position enabled a fourth void to be placed in this area, of the same *character* as the three below and immediately adjoining. These voids are mostly ' triangles with curved sides ', to borrow a phrase that is useful in dealing with Celtic art: they form a complete pattern on the Old Warden mirror,[3] and are characteristic of the style.

One point concerning our mirror-plate remains to be dealt with: the break in the major outline of the left half of the pattern (as seen by the viewer), where it is masked by the left bar and central boss of the handle. Close examination showed it *in the notch marked* X; thus the pattern was almost certainly completed on the curve required. Miss Cra'ster has drawn a side-view of the top of the handle, to show how the mirror-plate is slotted into it.

THE MIRROR HANDLE

The junction of the handle with the plate is shown from the side (FIG. 1); it is a massive casting (PLATE XXVII), consisting of three elements: the grip, the shaft, and a lower ring. The terminal knobbed coils of the former and the central boss are delicately wrought, as is the cross-moulding on either side; the detail of the heavily built shaft is less attractive. It is very similar to the Colchester handle,[4] but later and less refined.

The placing of our handle in relation to the kidney shape of the plate is axially correct: its relation to the pattern *on* the plate is interesting. It might appear that this was not considered by the engraver, who surely would not have expected one of his major curves to be masked by it. But a model of the handle, placed in the free sub-triangular space to the right, axially in line with the major diagonal of the pattern (the left S-curve), showed that such an axis for the mirror-plate was artistically as well as structurally impossible. The present setting is nearly parallel to the *true* axis of the asymmetric pattern, and 1 in. to the left.

The dots at top and bottom (FIG. 1) show the axis of the handle in relation to the pattern on the plate.[5]

[2] Related patterns will be seen on Pl. 56 of C. Fox, *Pattern and Purpose*: A. (Mayer), C. (Gibbs), P. (Colchester)—earlier; H. (Old Warden)—later.

[3] Fox *Pattern and Purpose*, Pl. 60, A. Here best seen at ' 4 o'clock ' on Plate XXVII, near the rim.

[4] *Pattern and Purpose*, Fig. 51.

[5] My paper on Celtic Mirror Handles in Britain, *Archaeologia Cambrensis*, 1948, gives the range of its type.

COMMENTARY

What, we must now ask, is the place of our mirror in the known sequence? It stems, of course, from the decorated three-roundel group, beginning with the Mayer and ending with the Desborough mirrors.[6] It is early in this sequence, for the design clearly was taken over at the symmetrical, Colchester phase, as the central theme and the handle suggest; but the whole structure has become distorted. Nevertheless, a new diagonal balance has emerged—in consequence the design has merit and can even be considered attractive. Completely asymmetric patterns are, of course, no embarrassment: there is a handle related to ours on the Old Warden mirror,[7] fixed to a plate patterned with three-sided voids of our type, with no axis anywhere; it is quite satisfactory.

'Taken over at the Colchester phase': the pronouncement does not preclude cross-fertilization in ideas between other local schools in Celtic Britain; the Colchester mirror, moreover, is but a fragment. There are features—the terminal nodes on each side of the Desborough mirror, the forms of the voids, one node, and the handle of Old Warden—that suggest wider sources for our mirror-school than one 'shop'.

Nevertheless, since the Colchester mirror and ours are related, and since one was found on the site of 'Camvlodvn', the capital town of the Trinovantes, the other not far away from it, both were probably made therein. The date A.D. 1–15 was suggested by myself for the Colchester mirror in 1948; A.D. 5–20 might then be our limits for this one. The market for such products was thus, we may hold, at the craftsman's door: the leading members of the tribe provided the intelligent appreciation and the wealth needed to promote and maintain high technical ability, and a flair for novel design, in the 'shops'.

This mirror then shows a familiar Celtic pattern, seen in part on the Colchester mirror, and taken over at this stage by a craftsman with new ideas: eccentricity and 'diagonal balance'—top-left to bottom-right—control his lay-out.[8] The result is exciting and interesting; the bizarre curvilinear structure moreover is most carefully designed, the void-forms being closely related. The piece is a notable work of art and its creator deserves our respect.

[6] *Pattern and Purpose*, Pl. 56.
[7] *Pattern and Purpose*, Pl. 56 H.
[8] This breakdown of a formal tradition is carried even further by the Old Warden mirror, *Pattern and Purpose*, Pl. 60.

Book Chronicle—*continued from p. 206.*

SIR MORTIMER WHEELER. *By* Ronald Clark. *pp.* 107, 9 *plates. London, Phoenix House,* 1960, 8*s.* 6*d.* The fourth in a series 'Living Biographies for Young People', the first three being Sir Vivian Fuchs, Sir John Cockcroft, and Peter May. The book ends with three blank pages entitled 'What Happens Next?'. We wish an Advisory Editor of ANTIQUITY that these may be filled with much.

THE ORIGINS OF ROME. *By* RAYMOND BLOCH. (Volume fifteen in the *Ancient Peoples and Places Series.*) *pp.* 212, 17 *line drawings,* 5 *maps. London, Thames and Hudson,* 1960. 30*s.* Professor Bloch, of the *École Pratique des Hautes Études* of Paris, enjoys controversial subjects, and his lively scholarship here provides a stimulating guide to the intricacies of Roman beginnings. The important archaeological evidence which now makes it possible to see Rome in its Italian background is examined and lavishly illustrated. It is also related to the literary record and to current advances in the study of the language, literature and religion of primitive Rome.

[*continued on p.* 239.

Notes and News

THE UNESCO CAMPAIGN FOR THE SAFEGUARDING OF THE SITES AND MONUMENTS OF ANCIENT NUBIA

The construction of the High Dam south of Aswan presents a challenge to Egyptologists without precedent, for we are faced with the fact that a large area of the Nile Valley rich in the relics of Egypt's ancient history will in a few years be totally destroyed. When the waters of the Nile are banked up by the High Dam, they will reach, between Aswan and Akasha in the Sudan, a height of about 600 ft. above sea-level, forming a great reservoir over 270 miles long and in some places 16 miles wide. All the great temples of Nubia, the Christian churches, the fortresses, the cemeteries and other ancient sites will be submerged and finally obliterated. The gigantic task of saving the temples and of recording and excavating the ancient settlements and cemeteries in the short time before the completion of the High Dam project in 1965 is beyond the present resources of both the United Arab Republic and the Republic of the Sudan. Consequently, both governments appealed to UNESCO, asking it in its turn to appeal to the nations of the world and their scientific institutions to assist in the preservation of this part of civilization which is indeed a heritage of humanity as a whole. This request was accepted by UNESCO, and on 8 March, 1960, the appeal was formally made public by the Director-General of UNESCO in Paris.

Already the response by governments, institutions and individuals has been most encouraging, and a Consultative Committee formed by the government of the United Arab Republic in co-operation with UNESCO has just completed its first session in Cairo, to organize and co-ordinate the various offers of assistance which, in one form or another, have come from all parts of the world. The answers to the appeal have been such that, during the coming winter, work on the various projects recommended by the UNESCO Committee of Experts in October 1959 will be in full swing.

With regard to the excavation of town sites and cemeteries, the situation in Egyptian Nubia is not as perilous as in the threatened area of the northern Sudan. With the building of the original Aswan Dam in the early 1900s, and its heightening between 1929–34, the Egyptian governments at that time organized what was known as the Archaeological Survey of Nubia. The first part of that work was undertaken by the late Dr George Reisner, and the second part, between 1929 and 1934, was completed under my direction. We excavated and recorded, so far as our knowledge carried us, all the ancient sites, all the cemeteries, settlements and fortresses in the threatened area; but of course we left alone sites which were high above the level to which the top of the new reservoir would reach. It is these sites on the higher levels of the valley which must now be excavated and recorded, and it is satisfactory to report that in consequence of the UNESCO appeal, concessions to excavate all the most important of these sites have been granted to expeditions coming from Egypt, U.S.A., Italy, Germany, U.S.S.R., Spain, Poland, France and Great Britain, who will commence work in the autumn of this year. In addition a ' sondage ' expedition to explore the areas between the individual concessions is being organized by the Egyptian Antiquities Service. So that, in general, the question of excavation in Egyptian Nubia is already settled.

But in Sudanese Nubia the task is much greater and the problems more acute. There, very little has been done hitherto, for the area between Wadi Halfa and Akasha was not threatened by the building of the original Aswan Dam. In Upper Nubia there has been a

certain amount of excavation, but due to the lack of urgency it was very limited and never on a really large scale. Now, with the construction of the High Dam, the waters will flood the whole reach of the Nile from Halfa to Akasha, so that a new archaeological survey is being organized by the Sudan Antiquities Service, which must explore the localities on both banks of the river for a distance of more than 90 miles. This area is of great historical importance, for it was here that the kings of the Middle Kingdom built a series of great fortress settlements, many of them still unexcavated. The Egypt Exploration Society is at present engaged on the complete excavation of Buhen, the largest of these fortresses, and it is to be hoped that other foreign missions will soon undertake similar work.

Apart from the excavation of all ancient sites in Egyptian and Sudanese Nubia, there remains the necessity for a comprehensive prehistoric survey which would also record the rock drawings and graffiti. Offers of help, financial and technical, have been made for this purpose, but as yet no detailed plan for its organization has been made.

But by far the greatest task facing the archaeologist in Nubia is the preservation of the temples, particularly those of Abu Simbel and Philae. With regard to the latter, the matter is not so urgent, for it will not be affected until the High Dam project has reached an advanced stage. Abu Simbel, on the other hand, will be damaged by the rising water when the first stage of the construction is completed—this should happen in little more than two years time. Already French engineers are carrying out the preliminary survey for the construction of the earth dam that will protect the temples. The cost of this earth dam cannot be estimated until this survey is completed, but it is certain that the cost will be very great, and we can only hope that the money for this great project will be forthcoming from international sources. In the meantime, the Egyptian Department of Antiquities have already started work on the transfer of the smaller temples of Debod and Taffeh, which are in immediate danger.

At the present time, the British contribution towards this great archaeological campaign is directed to the excavation by the Egypt Exploration Society of the important sites of Buhen in Sudanese Nubia and Ibrim in Egyptian Nubia. At the request of UNESCO, a national committee has been formed under the Presidency of His Grace the Duke of Devonshire, and it is hoped that through its efforts to obtain further financial support, the scope of the work of the Egypt Exploration Society may be considerably widened in both Egypt and the Sudan.

W. B. Emery

RADIO-CARBON DATES FROM WINDMILL HILL

The British Museum Research Laboratory has now completed the radio-carbon dating of charcoal samples collected from three separate layers during the 1957–8 excavations at the Neolithic camp on Windmill Hill (*WAM*, LVII (1959), 149–62; Antiquity, 128 (1958), 268–9). As explained below, each date refers to the contents of a layer which had accumulated over a considerable period of time.

Occupation surface sealed beneath the bank of the Outer Ditch (Cuttings V and VI): 2950 ± 150 B.C. (BM 73). The charcoal occurred as surface scatter, together with a small quantity from a hearth. Two, or possibly three, phases of occupation could be detected.

Primary silt in the ditches of the camp: 2570 ± 150 B.C. (BM 74). The term ' primary silt ' refers to the loose chalk rubble, occasionally interspersed with fallen turves or thin streaks of rainwash. As no large deposits of charcoal were found, the sample was made up of small amounts occurring at various levels in the primary silts of two cuttings in the Outer Ditch (IV and V) and one in the Middle Ditch (XII). There is reason to believe that these silts were all forming at the same time.

Late Neolithic surface in the Outer Ditch (Cutting V): 1540 ± 150 B.C. (BM 75). The

sample consisted of small amounts collected from varying levels within the layer. There had been some burrowing by moles in this upper layer, and although no charcoal from obviously disturbed areas was included, the possibility of contamination with younger material cannot be eliminated. However, it seems unnecessary to overweight this possibility when assessing the result.

It is very satisfactory that these dates from Windmill Hill form a series which accords not only with the facts of stratigraphy on the site itself but also with the rapidly accumulating evidence from Ireland and France that related branches of the Western Neolithic culture were already established in the 4th millennium B.C. (Watts, ANTIQUITY, 134 (1960), 111–13; Giot, *ibid.*, 147–8). The date range for the first two samples also neatly overlaps that for the Nutbane Long Barrow (Vatcher, ANTIQUITY, 132 (1959), 289). Since the material culture represented in the occupation antedating the camp seems already fully evolved, at least so far as food-production, pottery and flint industry are concerned, still earlier dates are to be expected for the initial and formative periods of the Windmill Hill culture.

By the middle of the 16th century we have reached the time when we can begin to reckon, on the archaeological evidence alone, in terms of half-centuries or less, so that inevitably the full span of possible dates offered by a radio-carbon measurement will not be acceptable. But any time within the upper half of the range for the Late Neolithic layer in the Outer Ditch—1690–1540 B.C.—would at present seem appropriate for most of the contents. (Some of the flint implements should probably be considered as survival rubbish.) Taking all the circumstances into account, one could hardly hope for a more satisfactory result.

ISOBEL SMITH

POTASSIUM–ARGON DATING

In August 1958 a discrete sensation was caused at the International Congress of Prehistoric and Proto-Historic Sciences at Hamburg, by the announcement of the first direct dates in years relating to the Lower Palaeolithic stages of human culture. These were obtained by the American physicists J. F. Evernden, G. H. Curtis, and R. Kistler[1] using a radioactive isotope of potassium—potassium-40.

Many readers of ANTIQUITY will probably be aware that previous estimates for the age of the various early stages of human cultural emergence in the archaeological record depended on two methods, both with very considerable limitations. On the one hand the radium method of dating for the older geological periods elaborated by Professor A. C. Holmes could be extended to give a very rough estimate of the total duration of the Pleistocene, during which all certain osteological and cultural traces of man make their first appearance. On the other hand, granted a number of somewhat fundamental assumptions regarding the causation of the Ice Ages, it was possible to propose a chronological framework on the basis of a theory of variations in the amount of solar radiations received by the earth over the last million years.

Finally by using the method of radio-carbon (directly applicable only to the final stages of the Middle and to the Upper Palaeolithic as defined in Europe and the Mediterranean) to calibrate certain other natural processes, it was possible to reach another—widely different—chronological scheme for the Lower Palaeolithic.

Apart from the inherent inconsistency of the results obtained by the last two mentioned methods, the theoretical limitations of these earlier datings of the Lower Palaeolithic are briefly as follows.

Despite the fact that in its newer form corrections and modifications have been intro-

[1] Evernden, J. F., Curtis, G. H., and Kistler, R (1958. *Quaternaria*, IV, 13–17).

duced by Professor Holmes himself and others, the radium dating method offers little chance of future improvements likely to make it available for dating within the bounds of the Pleistocene. Like the more recently evolved Carbon-14 and the potassium–argon method about to be described, the radium method derives its basis from the well-known physical fact that radioactive disintegration proceeds at a constant rate specific to the different isotopes involved, and unaffected by outside factors. The process is measured by the ' half-life ' of the isotope, that is to say the time required for half of any given quantity to disintegrate. In practice the measurement of the disintegration reached by any given sample (and hence its age from the time of formation) is obtained from a series of observations whose number provides a statistical estimate of the true degree of disintegration; that is to say an ' age ' subject to the normal margins of statistical error. Clearly, the slower the process of disintegration (e.g. the longer the 'half-life') the wider will be the statistical margin of error in years. In the case of radium the half-life is so immensely long that it is remotely unlikely that it will ever yield results which can be measured in units of less than a million years. Thus, valuable though the method is for the purpose of providing a general chronological framework of the greater episodes of biological evolution, it holds little promise for dating the climatic or cultural episodes within the relatively extremely short period of the Pleistocene.

The limitations attending astronomical estimates of Pleistocene time are of a different kind inherent both in the results and in the underlying theory. The latter is too well known to readers in this country from the works of Professor F. E. Zeuner to require more than the barest outline here; briefly, it is based on theoretical variations in the mean intensity of solar radiations reaching different parts of the earth's surface as a result of minor observable irregularities in the earth's orbit round the sun. The sum of the effect of these irregularities turns out to be a non-periodic function, that is to say that if it is expressed in the form of a graph representing the amount of radiation received by a given portion of the earth's surface, the graph presents a series of fluctuations whose pattern is never exactly repeated, however far back in time it is extrapolated. The irregularities in radiation were thought by W. Soergel as long ago as 1925 to show a certain parallelism to the geological record of advances and recessions of the Pleistocene ice sheets. This view is strongly upheld by Professor Zeuner, but has been much criticized by other writers. Both A. C. Holmes and R. F. Flint have emphasized that if the radiation fluctuations involved were really sufficient to bring on glaciations in the Pleistocene a further factor is required to explain why they did not produce similar results more often than is observed over the immense extent of earlier geological epochs.

Two further criticisms have been elaborated on the basis of radio-carbon readings. One is derived from a graph of sea-water temperature variations believed to cover a substantial portion of the Pleistocene and deduced from the istopic composition of cores obtained from the ocean bed. These cores consist of extremely fine stratified sediments and the temperatures deduced from the different layers are recorded in the remains of Foraminifera which formerly inhabited the surface of the sea and should reflect the temperature factor in Pleistocene climate. The upper layers only can be calibrated by radio-carbon owing to the relatively short time span of that method, but the earlier episodes are believed to have been accumulated at a comparable rate. The pattern of fluctuations so revealed has been claimed to show a certain similarity to the radiation curve, though correspondence is by no means perfect and appreciable uncertainty subsists as to which episodes correspond in the two series, especially as regards the earlier of these. Even in the later stages referable to the Last Glaciations progressive divergence seems to be apparent as one traces the two time-scales back in time. Thus, while both agree in dating the last truly glacial episode to 15–20,000 B.C.,

the first interstadial date obtained by calibrating the palaeotemperature curve with the theoretical curve of solar radiation (in a recalculated form) gives a date for its peak of just over 48,000 as opposed to the radio-carbon calibration of 35,000.

Direct dates of archaeological stages obtained by the application of radio-carbon and deductions from the radiation curve are even more at variance, when the former is used in the form originally calculated by M. Milankovitch. Thus the beginning of the Upper Palaeolithic associated with the second maximum of Würm in Europe is consistently given by radio-carbon as in the neighbourhood of 30,000 B.C., whereas on the solar radiation curve, according to Zeuner that phase should reach its climax at 70,000 B.C. If such discrepancies occur in estimates of the later Pleistocene climatic events it is at first sight even more difficult to place serious reliance on estimates by the same means of the very much earlier events associated with the Lower Palaeolithic. None the less it is only fair to add that the early dates about to be quoted are not quite so widely divergent from those predicted by solar radiation as the above observations might lead one to expect.

The new method, then, is based on a radioactive isotope of potassium which normally occurs in the proportion of 0·01 per cent; on decay it branches into calcium-40 and argon-40, with a half-life of $1{\cdot}3 \times 10^9$ years.

This relatively long half-life means that it can in fact be applied to the earlier periods of geological time, when the results can be compared directly with those of Professor Holmes's method; this has recently been attempted and shows a very reasonable degree of correspondence through with a certain systematic tendency to somewhat lower readings.[2] In applying the method to the very much shorter periods of time within the Pleistocene many technical difficulties had to be overcome for which we have principally to thank the three investigators quoted at the beginning of this article.

Among the most important dates published so far is one[3] of 2,600,000 for some period in the Upper Pliocene in Italy. Since no certain human artifacts antedate the beginning of the Pleistocene—very few indeed can be placed earlier than the beginning of the Middle Pleistocene—this figure is consonant with that quoted above of a million years for the beginning of the Pleistocene. A second date bearing on the same problem is that of 850,000 from a volcanic tuff overlying the traces of a very early glaciation in California equated on geological grounds with one of the Donau or Pre-Gunz stages of the European sequence.

While the above are relevant mainly to the background of human emergence a date of 400,000 from the Laacher See in southern Germany is correlated to an interstadial of the Mindel or Antepenultimate Glaciation, a period which is widely assigned to the earliest hand-axe cultures and to the earliest undoubtedly human fossil in Europe—the Mauer Jaw.

Less satisfactorily situated geologically, but none the less of considerable interest, is a date of 230,000 from Capranica in central Italy believed to correlate with the Middle Acheulean occupation site of Torre in Pietra, climatically of Great Interglacial age.

As this paper goes to press it is understood that results of even greater import are to be expected shortly from Olduvai Gorge, which may well be of cardinal importance in dating Zinjanthropus and establishing the role of Africa as a possible focus of origin of the great Hand-axe complex with all that that implies of the pattern of the primary spread of humanity.

C. B. M. McBURNEY

[2] Kulp, J. L. *et al.* (1960). ' Potassium – Argon and Rubidium – Strontium Ages of Some Granites from Britain and Eire '. *Nature,* **185**, 495.

[3] Oakley, K. P. (1959). ' Dating the Stages of Hominoid Evolution ', *The Leech*, vol. 28, p. 198.

TWO CARIAN WRECKS

The wreck of an 8th-century Byzantine ship and, to be precise, the cargo of a ship coming from Cyprus around 1250 B.C. lie in deep water off the Southern coast of Turkey. They have long been known to local sponge divers who spoke of them to members of the Izmir Diving Club. In 1258 Messrs Mustafa Kapkin and Rasim Divanli of Izmir took Mr Peter Throckmorton of New York and the writer to visit the Byzantine ship. Last year, the first-mentioned were joined by Mr Hakki Gultekin of the Izmir Archaeological Museum, whose interest in underwater finds had been aroused by the reports of the local Diving Club. Together, on board a privately owned American pleasure yacht, they visited the site of the Bronze Age ' wreck ' off Finike.

It was only possible for them to spend one day on this site, but during that time Mr Kapkin made an excellent sketch-plan of the lie and disposition of the cargo and sufficient was lifted to show its very great interest and importance.

At this point the pleasure yacht returned to America and the above mentioned to their base at Bodrum. Through the imaginative measures taken by Mr Gultekin, the Turkish Government consented to the fine Crusader Castle of Bodrum being made into a museum for underwater finds. The objects lifted from the Bronze Age ship were taken to this new museum, where the writer (who had returned to make a plan of the Byzantine wreck) was able to draw them.

The Bronze Age ship sank in 32 m. of water and unfortunately came to rest on a rocky bottom where sand only 6 in. deep lay in the cavities. The result was, that although some interesting organic matter was preserved in this sand, the main structure of the ship, as is always the case in these conditions, had disintegrated.

It is possible to say from this brief examination, that the ship carried a cargo of about 16 ox-hide copper ingots bearing the Cypriot hallmark, and at least one bun-shaped ingot. A mould from which a similar ingot to the latter could have been cast, was found by Dr Dikaios at Enkomi and is dated to the 13th century B.C., late Cypriot II B. Mr Kapkin sketched, but did not lift, ' half a football made of bronze ', possibly an oval ingot. There was also a flat, hatchet-shaped object similar to some found in the Enkomi Hoard, which might be yet another type of ingot.

Half a double-headed axe was raised; unlike the other bronze objects which, though concreted, had a fairly solid metal core, this axe looked like steatite and was friable which possibly indicated that it had a heavy alloy of tin. Since the shaft hole was large it is likely to have been an implement, not an ingot.

Other metal objects lifted were reminiscent of the Enkomi Hoard and included some pike-like agricultural implements and ploughshares; also a spatula-shaped bronze blade 60 cm. long, which was well enough preserved to show marks of burnishing. Small fragments found in the sand point to the presence of other metals, possibly silver and lead. A bar of bronze measuring 1·10 m. and 7 cm. in diameter was joined to the rock at either end by concretions, and was not lifted.

The sponge divers report that two years ago they found a couple of bronze caskets near the ingots which they brought up and opened on the deck of their caique. Finding that they contained ' only black stuff ' (silver ?) they threw them back. They also say they have seen ' two bronze couches ' in the vicinity.

Very few sherds were found. These lay in the shallow sand below some of the ingots. The most interesting was a handle from a stirrup jar with two incised crosses, one in the centre of the false neck. The crosses could be Cypro-Minoan characters. Well-preserved rope adhered to this handle, and another length of 1·50 m of rope was buried in the sand nearby, together with some wood. Other sherds included a small piece of Mycenean

Fig. 1. Plan of the Byzantine Wreck. (1) Concreted anchors, probably with movable flukes and rings on stock, shank and crown. (2) Edge of concreted metal emerging from the sand. (3) Same as two. (4) Probe made outwards from between the anchors. Pottery was struck, then a softish substance like wood running for about 40 cms. parallel with a line joining the anchors. (5) Heavily concreted kedge anchor. (6) Bronze pipe loose on sand. (7) Concreted iron (?) sticking out of sand at an angle of 45 degrees. (8) Galley area. (9) Wood, 30 cms. below sand; part of a beam lifted. (10) Roof tiles and sherds. (11) Long, pointed amphora. (12) Metal object emerging from sand and sherds. (13) Metal object emerging from sand. (14) Sponge diver reports he took 2 pieces of lead from here. (15) Earthenware cooking pot. (16) Vase. (17) Amphora neck. (18–22) Amphorae of representative types. (23) Copper tray concreted. (24) Floor tiles. (25) Roof tiles. (26) Probe struck pottery and wood at X. (Plan by Honor Frost.)

painted ware, the spout of a larger stirrup jar, the neck of a vase and part of a similar vase with a side handle. These and other small sherds were discoloured. If this blackening proves to be burning it may explain the wreck of the ship. The rope suggests a reason for lack of sherds; the few that were found may have been tied to the heavy cargo of ingots. Since the current at this spot runs at about two knots, the light deck cargo of stirrup jars and other pottery would have been carried away as the ship sank. When it is possible to make further investigations, they may be found down-current, lodged under shelves of rock.

By contrast to this dispersed cargo, the Byzantine ship carrying globular amphorae can be described as the perfect wreck. She lies on a sandy bottom; a group of heavily concreted spare anchors and a kedge anchor indicate her bows at a depth of 33·3 m., while her cargo of amphorae run down the slope to a depth of 38·3 m., and possibly further below the level of the sand. (Fig. 1).

The galley structure was built aft and to port. The roof tiles lie much as they fell above the floor tiles; a heavily concreted bronze tray was found in association. Similar globular amphorae were found on a site in Chios and were dated to 750 A.D. With careful excavation it may be possible to trace the contents of the amphorae carried on this ship; those buried below the sand may have stoppers and the lack of oxygen at this level will favour the preservation of organic matter.

To the west of the galley area, a wooden beam, 30 cm. below the level of the sand was traced for a length of 1·50 metres. About 40 cm. was raised, showing it to be a timber square in section with two protected surfaces. What material, whether paint or leather, covered these surfaces, has yet to be ascertained. Between this wood and the anchors (see plan) a piece of bronze piping lies loose on the surface. Until it is lifted and examined it is not possible to say whether it was used for drainage or some other purpose. These and many other questions, pertaining to the actual structure of Byzantine ships are raised by this wreck. It would also be an ideal site on which to develop a technique of underwater examination.

HONOR FROST

MALTA AND THE MEDITERRANEAN

In his review of my book on Malta (ANTIQUITY, 1960, 132) Professor Bernabò Brea expresses opinions which differed strongly from mine as set out there on a number of important issues. Since the views of so eminent an authority on Mediterranean prehistory must carry great weight, I feel that it is desirable that I should state that I find his views in the main inacceptable. A full discussion of all the problems which he has raised is obviously impossible here, so I shall confine myself to a few of the most important points.

While accepting my culture-sequence and my suggestion as to the probable way in which the temple-architecture originated, Professor Brea makes three main criticisms of my position, as follows:

(1) There is a cultural and chronological break between the Għar Dalam phase and the rest of the sequence.

(2) The last phase of Period I ends several hundred years before the date of 1500/1450 B.C. which I propose and all the earlier phases should be corespondingly updated.

(3) The culture of the Period I temple-builders was not isolated. It was the product of people connected with the Early Bronze Age inhabitants of the Aegean, who came to Malta at about the time that similar groups began to arrive in Sicily, made of the Maltese islands a most important base for sea trade, and were probably responsible for starting the fashion of megalithic architecture around the shores of the Western Mediterranean.

I shall take these points in turn and try to answer them.

(1) I find it difficult to grant the existence of this cultural hiatus. There is no real ground for the large assumption that some of the temples re-used much older sacred sites. Technically, the pottery of the Mġarr phase seems to me to represent a simple development from the Għar Dalam wares. In one case the impressed decoration of the Għar Dalam and the cut-out technique of the Mġarr wares are combined on the same sherd. The few sherds of Diana type found are certainly from imports—there was no Diana phase in Malta! The only one of these sherds whose stratification is known came from a level with mixed Zebbuġ and Ġgantija phase pottery, and I see no reason for thinking that this does not represent more or less its true context.

(2) The dating of the Period I phases is admittedly very difficult. The earlier phases were dated by me mainly on comparisons established with Sicilian cultures, so that if Professor Brea raises his dates for the latter, as he seems to have done, the Maltese dates must follow, and I can see no objection to this. The dating of the final (Tarxien) phase, however, is much more complex. A fixed point is the dating of the first phase of Period II (Tarxien Cemetery) to the 14th century B.C. by the presence in it of a segmented blue faience bead, and of a necklace of hundreds of tiny disc beads of faience and ostrich shell which has an exact parallel in a contemporary *tholos* in Greece. The pottery of the Cemetery shows no development and the few traces of the culture (i.e. the score of dolmens, etc.) found elsewhere in the islands does not seem to indicate the duration of 300–400 years which Professor Brea would now claim for it. 100–150 years would seem to me to be more likely. Of course, there could, as Zammit thought, have been a time after the end of Period I when the islands were uninhabited, though I do not think it very likely, and certainly they would scarcely have remained so for long.

A *terminus post quem* for the beginning of the Tarxien phase itself is given by the volute motif so common on its pottery, which is clearly imitated from the stylized imitation of veined stone which is popular as a decoration on Minoan cups from M.M. II onwards. There are parallels in the pot-shapes with the S. Ippolito pottery of Sicily, which shows that it must be at any rate partly contemporary with this culture, but I feel certain that Bernarbò Brea is wrong in ruling out an overlap with the Castelluccio phase also. This is indicated by a number of things, especially the similarities between the oculus spirals carved on slabs in the Maltese temples of this phase and those on two Castelluccio tombs, and the occurrence in both the Tarxien and Castelluccio cultures of rare examples of grey ware with *pointillé* decoration—possibly also the bossed bone plaque from Malta if it really belongs to the Tarxien phase and not the Tarxien Cemetery phase, as seems probable. Such an overlap would make the continuance of the Tarxien phase till at least *c.* 1600 a virtual certainty. I agree that there is no absolute necessity to postulate an overlap with L.M. and L.H. to explain the Aegean parallels offered by the Tarxien culture, which can all be with M.M. (especially M.M. III). From this point of view a lower date of 1600 B.C. for the Tarxien phase would be as acceptable as one of 1500 B.C. I would point out, though, that we have as yet no *direct* evidence of Minoan trading with the central Mediterranean prior to *c.* 1500 B.C., though of course this may come.

(3) In calling the Maltese temple culture 'isolated' I did not mean to suggest that it was without foreign contacts; indeed, as I have pointed out, there is good evidence that this was not so. It seems to me that the turning point of the culture was brought about by the influence of contact with East Mediterranean civilization at the end of the Ġgantija phase, and the Tarxien style of pottery decoration evidently begins as a self-conscious, rather clumsy attempt to imitate the elegance of a foreign style. The isolation was rather psychological. An ancestor cult does not produce missionaries, and I do not see the Maltese

either as missionaries or colonists. If they really started the megalithic architecture of Western Europe, why is it that there are only tombs and no megalithic temples elsewhere? And why do these tombs show only the vaguest and most general resemblances to the Maltese temples? Finally, why do the earliest show less resemblance than the more developed?

There is no instance we can point to of any Maltese cult-object or pot having been exported, nor of any group of Maltese emigrating to another part of the West Mediterranean. As to the islands having been an important trading centre, this is purely hypothetical. The Lipari islands became an important trading centre originally because of their richness in obsidian, whatever later developments there were. In Malta there was no such reason for trade to build up and the Maltese islands lie further off the Second Millennium trade routes. Malta only became of international significance as the strategic base of maritime empires and there is no evidence that it was ever this for the Minoans or the Myceneans. The idea of widespread Maltese influence in the prehistoric West Mediterranean is attractive as simplifying the problem of megalithic origins, but unfortunately there is just no evidence to support it!

JOHN D. EVANS

SIR LEONARD WOOLLEY MEMORIAL FUND

An editorial notice about the death of Sir Leonard Woolley has appeared in the preceding number of ANTIQUITY. Here we draw the attention of readers to the establishment of a Woolley Memorial Fund, and it is hoped that many may be attracted to support it.

It will be seen from the appeal hereunder that a dish made by the Worcester Royal Porcelain Company was designed as a special memento of his work at Ur, which marked the culminating achievement of a lifetime devoted to excavation. Many other expeditions were also directed by him, but it is particularly with Ur that his name will be associated. The commemorative dish, a triumph of modern craftsmanship, is ornamented with a reproduction of the famous gold dagger and sheath, and the wreath of golden willow and beech-leaves which were found in the Royal Cemetery.

It is curious to reflect that when the dagger was discovered, together with its lattice-work sheath, one of the leading archaeologists of the day pronounced it to be a work of the Renaissance—about 16th century A.D.—and it took some little time to realize that Woolley had in fact found the fine flowering of Sumerian civilization which could not have flourished later than the 3rd millennium B.C. Long discussions about the chronology ensued, and it is only recently, after years of comparative investigation on other sites, that the date of this gold-work has been satisfactorily established as having been the product of Sumerian smiths round about 2600 B.C.

The significance of Woolley's contributions to our knowledge of Sumerian civilization, and the bearing of subsequent discoveries on his work at Ur will be discussed in the autumn at a memorial lecture given by the writer. This is to be held at a joint meeting of the Royal Geographical Society and the British School of Archaeology in Iraq on 7 November; members of these societies will receive invitations to it.

I had the good fortune to work with Woolley at Ur for six years from 1925–30, and was present throughout the work on the Royal Cemetery, an experience which no one could forget. The abundance of precious material of every description that poured in day after day to the expedition house from the royal graves was extraordinary; the task of preliminary cleaning, maintenance, and cataloguing was one that taxed our small expeditions of not more than five or six persons to the utmost. It was Woolley's aptitude for organization, his ingenuity in improvising devices for conserving highly fragile material that was responsible

for the salvaging of much that would otherwise have perished, and it has to be remembered that at that time laboratory work concerned with ancient materials was still in its infancy. Posterity owes him a debt for this as much as for any other achievement, and it is fortunate that he was endowed with a practical imagination capable of foreseeing the sort of methods which would have to be used in solving problems which were then entirely new. The story of his life deserves to be written in detail some day and must surely be an incentive to every new generation concerned with rediscovering the past.

It is hoped that there will be as wide a response as possible to the appeal which has already been circulated to many persons, and we wish to thank the Editor of ANTIQUITY for allowing us space in this number to reprint it here.

By the death of Sir Leonard Woolley on 20 February, 1960, two months before his eightieth birthday, archaeology has suffered a great loss. He had been associated with the British School of Archaeology in Iraq since its foundation in 1932, and it is felt that his friends and colleagues may wish to join in some tribute to his work.

It is therefore proposed to establish a Woolley Memorial Fund which will be held by the School and used to afford young students opportunities for the study of archaeology in Iraq and neighbouring countries. The same fund could also be used for the publication of archaeological articles. We feel that these proposals would have appealed strongly to Sir Leonard himself.

A special memorial number of the journal *Iraq* (vol. XXII), wholly devoted to articles on the discoveries at Ur, will be published this year. The names of subscribers to the Memorial Fund will be printed in this and in subsequent volumes.

Cheques or postal orders should be sent to: *Miss G. C. Talbot, Assistant Secretary, British School of Archaeology in Iraq, Institute of Archaeology*, 31–34 *Gordon Square, London, W.C.*1., who will send an official receipt.

M. E. L. MALLOWAN.

Professor Mallowan adds: This appeal supersedes one which had been sent out to some colleagues and friends of the late Sir Leonard Woolley shortly before his death. It had then been intended to present him on his eightieth birthday with a dish made by the Worcester Royal Porcelain Company, and subscribers were invited to contribute towards its cost. If any subscribers to the Memorial Fund also wish to be associated with this special tribute, will they kindly express their assent when sending their contribution. This beautiful piece, suitably inscribed on the back, now held by the British School, will be publicly exhibited on some appropriate occasion, and perhaps presented to one of the institutions associated with his career.

BRITISH ARCHAEOLOGY ABROAD

The Annual Report of the British Academy for the year 1959–60 contains an interim report from Mr G. Lankester Harding on the archaeological survey which, on representations submitted by the Academy, the Aden Colony and Protectorate had invited him to undertake in those territories, and news of the progress of the establishment of the British Institute of History and Archaeology in East Africa. This Institute, already briefly referred to in these pages (ANTIQUITY, 1959, 137) has now acquired a building at Dar-es-Salaam as its main headquarters; Mr Richard Goodchild, at present Controller of Antiquities in Cyrenaica, has been appointed Director from 1 September, 1960, and two studentships, each of the value of £600 plus travelling expenses, have been advertised in connexion with the Institute. The Report also contains the good news that the British Academy and the British Council have arranged for an experimental period of one year from 1 September, 1960, that the Academy shall appoint an Archaeological Attaché to the British Council in Persia ' with a view both to widening the scope of the Council and to preparing the way

for possible future development in the historical and archaeological field.' Mr David Stronach has been appointed to this experimental post and we wish him well. The Report goes on to say that ' it is hoped that this tentative collaboration with the British Council will not only open up a new approach for British students working in the Middle East, but will also serve as a pattern for similar interim developments elsewhere ', and we are sure that everyone will re-echo this hope.

BRONZE AGE CONFERENCE

Mr Paul Ashbee, Secretary of the Council for British Archaeology Research Committee on the Neolithic and Bronze Ages asks us to remind readers that a week-end Conference on ' Problems Relating to the British Bronze Age ' will be held at the Institute of Archaeology of the University of London, 31–34 Gordon Square, London, W.C.1, from Friday evening to Sunday afternoon 16–18 December. The opening address will be given by Professor C. F. C. Hawkes on ' A Scheme for the British Bronze Age '. There will be an exhibition of material from excavations and field-work in the last ten years. Those interested should write to the *Council for British Archaeology*, 10 *Bolton Gardens, London, S.W.*5.

CIVITAS: ANOTHER MYTH

In his review of *Town and Country in Roman Britain* (ANTIQUITY, 1959, 68), Mr Sheppard Frere takes Mr Rivet to task for saying that ' Venta Belgarum in a sense *was* the *civitas Belgarum.*'[1] Mr Frere goes on to say ' but Venta was no more than a *vicus* in the *civitas.*'[2] In doing so I think he is surely perpetuating another of those myths against which he inveighs elsewhere in his review.

The myth derives from those studies of Roman Gaul[3] which assumed that, because their territories were relatively large, the *civitates* of Gaul must have been differently organized from other cities of the Roman world. In this view, the Gallic *civitas* was a sort of county council, with no town on its territory enjoying a status higher than that of a municipal borough or urban district.

It may be doubted whether a Roman lawyer would have been able to understand this. The word *civitas* has two meanings, as a legal term: (1) a self-governing city, the exact equivalent of the Greek πόλις; (2) citizenship, as in *civitas Romana*, the citizenship of the *city* of Rome. In the ancient world, the only form of civilized life was city life, and the only form of community of which one could call oneself a *civis*, in the legal sense, was a city. The two meanings merge in the definition given by Aulus Gellius[4] (he believed that he was quoting Verrius Flaccus) *civitatem et pro loco et oppido et pro iure omnium et pro hominum multitudine dici*: the meaning is clear, whether or not *omnium* should be amended to, e.g. *civium.*

Each *civitas* of Gaul (and of Britain) was therefore presumably a city, and like all true cities, each will have consisted of a single physical city, surrounded by its *territorium*, the whole under the control of its magistrates, council and assembly, the last no doubt usually of little importance. The physical centre cannot have been a *vicus* of the *civitas.* This is tantamount to saying that it was a *vicus* of itself, which is absurd. *Vici* will of course have existed, as elements subordinate to the city—they will have been either villages on the

[1] *Ibid.*, 65.
[2] Similarly Mr Stevens, *JRS*, XLIX, 188.
[3] For bibliography, Pauly-Wissowa, *Realencyclopädie*, Supplement, Band I, 301.
[4] Aulus Gellius, *Noctes Atticae*, XVIII, 7, 5.

territory or administrative subdivisions of the city itself, enjoying only a small measure of self-government.

Thus the *vicani* of Agedincum who appear in ILS 7049 are merely the inhabitants of all or some of the wards of the city, and the use of the word need occasion no more surprise than in ILS 6663, where it is applied formally to the citizens of the Roman colony of Ariminum. Much more will need to be known of the internal organization of the cities of Gaul before we can comment intelligibly on the *aediles* (and other dignitaries) mentioned on this stone: it cannot be held to prove that Agedincum was merely a *vicus* of the *civitas Senonum*.

As far as Britain is concerned, it now seems probable that the *vicus* attested at Brough on Humber by an inscription dating to the reign of Antoninus Pius[5] has nothing to do with the organization of any city which may have been founded there. Recent excavations have shown that the site was still in military occupation in the early 2nd century[6]. The *vicus* is presumably the extra-mural settlement, surviving after the removal of the garrison, whether or not it ever later achieved city status: so long as it was a *vicus* under military control (as *vici* in the military area normally were) it has no relevance to city government: it may have been eventually incorporated in the territory of another city.

There is no other evidence from Britain to suggest that its *civitates* were in any way abnormal. The policy of Romanization of which Tacitus claims that Agricola was so important an instrument was a very real attempt to introduce city life, but an attempt based on the foundation of true cities, not of forms of local government alien to the Roman mind. The development of the villa can only be properly appreciated in this light: the fundamental study is *TCRB*, chapter 5, especially pp. 103–13. The *civitates* differed only in status from Roman colonies and *municipia*. It follows that such mythical terms as ' cantonal capital ' and ' tribal capital ' need to be removed from the Roman-British vocabulary. It is better to call a city a city: Venta Belgarum *was* the *civitas Belgarum*.

J. C. Mann

[5] *JRS* xxviii, 199 and pl. xxxiii, 1.
[6] *JRS*, xlix, 109.

NEW JOURNALS

The first number of *The Journal of African History*, edited by R. A. Oliver and J. D. Fage, has been published by the Cambridge University Press at 20*s*. and contains ten articles varying from James Walton on ' Patterned Walling in African Folk Building ' to N. R. Bennett on ' Christian and Negro Slavery in Eighteenth-century North Africa ', and the review is of twenty-six books. *Antiquités Nationales et Internationales* is the title of a small journal of 24 pp. (5 new French francs) produced by the Centre d'Études Pré- et Proto-historiques de l'École Pratique des Hautes Études (Sorbonne) with the collaboration of the Musée des Antiquités Nationales, and edited by Monsieur André Varagnac, the Conservateur-en-Chef of the Musée. The contributions are short, and there are notes which will keep all informed of what is going on in the Musée at St-Germain, including the fact that new lighting is being installed and that a Société des Amis du Musée des Antiquités Nationales et du Château de St-Germain has been created under the presidency of Professor Piveteau.

Reviews

EARLY INDIA AND PAKISTAN. *By* Sir Mortimer Wheeler. (Vol. 12 in the series 'Ancient Peoples and Places.') *Thames and Hudson, London, 1959. pp. 241, 57 photographs, 25 line drawings and 7 maps. 25s.*

Ten years ago Professor Stuart Piggott laid those interested in Indian archaeology under a great debt by publishing an excellent account of India's past, from prehistoric times down to *c.* 1000 B.C. in his *Prehistoric India*. But with the passage of a decade, wherein several important discoveries have been made, particularly those relating to Middle Stone Age tools, the extension of the Harappā culture, the identification of the Painted Grey Ware Culture, the chalcolithic cultures of central and western India, etc., his book has become out of date. Thus, embodying these new discoveries, came out, in 1956, *The Personality of India* by Dr B. Subbarao. This was followed, in 1958, by Col. D. H. Gordon's *The Prehistoric Background of Indian Culture*. Subbarao's book has since been revised and published in a second edition (1958), which, incidentally, speaks of the usefulness of Gordon's book.

Against this background comes Sir Mortimer's *Early India and Pakistan*. And let it be said at once that, far from suffering from any contrast, the book may well be said to outshine all its predecessors: Piggott's, chiefly by its up-to-date contents; Subbarao's by its catchy language and superb illustrations; and Gordon's, by a clear-cut exposition of the subject-matter, apart from a few other points here and there.

Early India and Pakistan is divided into nine chapters, of which, the first two, entitled ' The Scene ' and ' Time ', deal respectively with the geography of the country and the limits in time of the survey, including a detailed note on the Painted Grey, Northern Black Polished and Rouletted Wares, all of which, during recent researches, have proved themselves to be of special chronological importance. The next two chapters, III and IV, captioned in a novel-like fashion, ' Stones ' and ' More Stones ', treat respectively of the Old Stone Age and the Middle and Late Stone Ages. Chapters V and VI deal with the dawn and early development of civilization in the two great river-valleys of the north, the Indus and the Ganges, while the following two chapters, VII and VIII, do almost the same thing for central and south India, in the latter case the chief emphasis being on the megaliths, which, by their profusion in that area, deserve the detailed treatment that has been given to them. The last chapter, IX, is a kind of epilogue intended primarily to bring pre- and protohistory to the doors of history and thus to introduce the reader to some well-known historical figures like Darius, Alexander, Aśoka, etc.

Written in a masterly style, characteristically Wheelerian, and suitable alike for the scholar and the layman, the book incorporates the latest discoveries, and shows the author's acquaintance with the latest publications in India. At the end are given ample references to allow the serious student to probe deeper into the subject. The illustrations are excellent, no praise being too much for the plates in particular.

A present-day Indian archaeologist is, in a way, handicapped in criticizing, or differing from, anything done or said by Sir Mortimer, for he owes, to a considerable degree, either directly or indirectly, his methods and acumen to that savant, who gave a shape to Indian archaeology during four years of the last decade (1944–48) when he was in India. The present reviewer, one of the direct associates of Wheeler-in-India, has, however, ventured to overcome this handicap and the Indian's traditional reverence of his *guru* when he records the following criticism, which, he is confident, will be taken by the ever-magnani-

mous Sir Mortimer in the spirit in which it is meant. One of the points of criticism relates to Sir Mortimer's tendency to rush to conclusions, or throw out suggestions of far-reaching consequence, although in reality there may not exist any basis for them. This may be illustrated by a few examples.

In 1948, while discussing the date of the Megalithic Culture at Brahmagiri, Sir Mortimer stated: ' I find it easier to suppose that the northward move of the megaliths occurred later, in the chaos which followed the death of Aśoka, *c.* 236 B.C., when the Mauryan empire melted away and a Dark Age settled upon the Deccan for over three centuries.'[1] In the present book (p. 164), however, he says: 'At the risk of complicating the matter with a gratuitous historical intrusion, I here add the suggestion that the southward extension of the Mauryan Empire into the Peninsula at the beginning of the 3rd century B.C., probably in the time of Ashoka's father, Bindusāra, provides an apposite context. The sharp and sudden difference at Brahmagiri between the magnificently equipped megalith-builders and their relatively primitive stone-using predecessors is exactly consonant with a tremendous historical event of the kind. On this view, the three copies of Ashoka's Minor Rock-edict at Brahmagiri were appropriately addressed to the southernmost colonists of his father's empire, and not, as I had previously conjectured, to their bucolic precursors.' But if one were to examine the lines of argument adopted on the two occasions, one would find that in each case the conclusion is subjective rather than objective and is apparently the outcome of a preconceived idea and an attempt to adjust the data in a particular way, there being in reality no archaeological evidence to associate the movement of the Megalithic people with either Aśoka or his predecessors or successors.

On another occasion, this very tendency to ' fit in ' things into a preconceived historical perspective has led Sir Mortimer to contradict his own writing before the ink could dry. The reference is to the dating of the Northern Black Polished Ware at Taxila, discussed on pp. 30–31 of the book under review. In the earlier part of the paragraph, Sir Mortimer says: ' Of these (i.e. of twenty sherds of the Ware found at Taxila), eighteen came from the earliest of the successive sites of the town, the Bhir Mound, which was founded late in the 6th or early in the 5th century B.C. and is thought to have ended about 180 B.C. It was visited by Alexander the Great in 326 B.C., when the average level was about 6 or 7 ft. below the present surface. It is noteworthy that sixteen of the Bhir Mound sherds occurred at depths between 7 and 13 ft., only two being higher than 7 ft. (one at 4 ft. 10 in., and one at 6 ft. 2 in.). Since Taxila was not dug stratigraphically in the modern use of that term, these depths are unreliable guides, but consistently with them, one of the two sherds found on the succeeding site of Taxila at Sirkap occurred at 18 ft. below the surface, and should therefore be early in the history of that site, that is to say, in the first half of the 2nd century B.C. On this showing, the N.B.P. Ware may be ascribed to 5th–2nd centuries B.C.' Proceeding hardly a hundred words ahead, he adds: ' On recent observations I am indeed inclined to suspect that in the far north-west at Chārsada near Peshawar (where a dozen stratified sherds of this fabric were found in 1958) [*but no date is given—reviewer*], at Udegram in Swat (where Professor G. Tucci's expedition has found a sherd in a milieu attributed to the 3rd century B.C.), and even at Taxila itself, its arrival should mostly be equated with the spread of the Mauryan dominion from the Ganges to these parts after 323 B.C.' One really wonders how Sir Mortimer could press the point that ' even at Taxila ' the ' arrival ' of the NBP Ware should be dated ' after 323 B.C.' in the face of the evidence that 80 per cent of the sherds of that site came from pre-Alexandrian levels, the earliest going back, on Sir Mortimer's own reckoning, to the 5th century B.C.! Has all this not

[1] *Ancient India*, 4, 202.

resulted from a desire to ' fit in ' the archaeological data into a preconceived historical setting ?

One is, however, glad to note that at least on one occasion Sir Mortimer has seen the necessity of withdrawing the unfitting historical garb from its archaeological skeleton. In the course of his report on the 1946 excavations at Harappā, Sir Mortimer had exclaimed that ' on circumstantial evidence, Indra stood accused ' of invading and destroying the city of Harappā.[2] He sought to identify the superimposed Cemetery H culture with the Aryans. There was, however, hardly any justification for such an assumption, the only clue that seems to have swept away Sir Mortimer being that Harappā had a fortified citadel and that Indra is referred to as *Purandara*, ' destroyer of forts ', in the Vedic literature. As this identification was of some importance, the reviewer had to point out the fallacy involved in the conclusion, viz. that since there was a substantial gap between the Harappan and Cemetery H occupations, as shown by Sir Mortimer's own excavations, the Cemetery H people could not be taken as ' invaders ', for by that time the ' invaded ' had long gone into oblivion. Then, again, one would expect the occurrence of the Cemetery H culture at at least a few sites known from literature or tradition to have been associated with the Aryans.[3] In the book under review (pp. 114–45), Sir Mortimer has gracefully withdrawn his theory and has made only a matter-of-fact statement: ' This Cemetery H Culture has at present been identified on only two other sites (in Bahāwalpur), and little is known of it save that it would appear to have post-dated the Indus Civilization by an appreciable interval.'

Lastly, the reviewer would like to take up an example of Sir Mortimer's wishful thinking. On p. 170 of the book under review, he says: ' It is just possible that Ashoka had Seleukid blood in his veins.' One does not know what evidence Sir Mortimer has to substantiate this view, but the known facts of history tend to show that it is almost impossible. Sir Mortimer's view evidently has its root in the *jus connubii* that was established between Seleukos Nikator and Chandragupta Maurya in *c.* 305 B.C. In the first place, scholars are not agreed that this must be interpreted to mean only that a daughter of Seleukos was wedded to Chandragupta Maurya.[4] However, even if it were accepted that Seleukos's daughter was one of the wives of Chandragupta Maurya, the question is whether Aśoka, a grandson of Chandragupta, had actually descended from that lady. This could be possible only if either of Aśoka's parents was born of that lady. Now, according to one tradition (*Aśokāvadānamālā*), Aśoka's mother was one Subhadrāngī, who was the daughter of a Brahman of Champā,[5] while according to another tradition (*Mahāvaṁsatīkā*), her name was Dharmā and she was a Kshatriya by caste.[6] Whatever be the correct name and caste of Aśoka's mother one thing at least seems to be clear that she was not an offspring of the Greek lady. But if in spite of this one argues that, as the traditions are not unanimous about the identification of Aśoka's mother, there is still the possibility of her having been Seleukos's grand-daughter, one should pause to think of the implications of such a stand. This would mean that Bindusāra was married to his step-sister, a position absolutely unwarranted by any record or tradition and indeed unimaginable in the concerned cultural setting. The question thus finally reduces itself to whether or not Aśoka's father, Bindusāra, was born of that lady. Bindusāra is known to have ascended the throne in *c.* 297 B.C. Thus, if it is

[2] *Ancient India*, **3**, 82.
[3] *Ancient India*, **10/11**, 151.
[4] Cf. *Cambridge History of India*, I (1922), 431.
[5] V. A. Smith, *Asoka*, 3rd edn, Oxford, 1920.
[6] R. K. Mookerji, *Asoka*, 2nd edn, Delhi, 1955.

assumed that he was born of Seleukos's daughter, who was married to Chandragupta in 305 B.C., the inevitable conclusion would be that at the time of ascending the throne, Bindusāra was not more than eight years old. Such a position, however, is not corroborated by any evidence—Indian, Ceylonese or Greek. On the other hand, the following circumstantial evidence suggests that Bindusāra must have been born prior to 305 B.C. Aśoka is known to have acted as a Viceroy at Ujjain as well as at Taxila before ascending the throne in 273 B.C. He is, therefore, likely to have been at least twenty years old at the time of his accession, which would mean that he was born not later than *c.* 293 B.C. It is also well known that Aśoka was not the eldest issue of his father. This would mean that Bindusāra was already a father by *c.* 295 B.C. Under the circumstances, it is difficult to imagine how Bindusāra could be born of the daughter of Seleukos who, if at all, could have been married to Chandragupta only as late as 305 B.C. To argue for the contrary would mean to concede that Bindusāra became a father at the age of ten!

The reviewer's intention in raising the above points is not to argue for or against any particular hypothesis. All that he wants is to urge that subjectivity should be kept far, far away from all archaeological interpretations, more particularly when issues of great historical significance are involved. While this precaution is necessary in the case of us all, it is certainly much more so in the case of a writer of the eminence of Sir Mortimer, whose every word carries weight with both the serious student as well as the amateur reader. Has not the leader much greater responsibility than the led ?

May it, however, be stated once again that the book is indeed a very welcome asset and students of Indian archaeology will always remain grateful to Sir Mortimer for the gift ?

B. B. Lal

DER HUSTERKNUPP: EINE NIEDERRHEINISCHE BURGANLAGE DES FRÜHEN MITTELALTERS. *By* Adolf Herrnbrodt. *Beihefte der Bonner Jahrbücher, Band* 6. *Böhlau Verlag, Köln,* 1958. *pp. xi* × 220, 78 *figures,* 44 *plates,* 10 *folders in pocket.* DM 22.50.

This book records the most complete excavation of a motte we have yet seen.

Der Husterknupp was a motte-and-bailey earthwork which lay beside the river Erft about 25 km. south-west of Düsseldorf. Impending destruction occasioned its excavation in 1949–51.

Dr Herrnbrodt distinguishes four main periods in the site's medieval development, and subdivides one of these. Already the Husterknupp is being spoken of as a microcosmic demonstration of the motte's evolution; but other interpretations are equally possible.

In period I (late 9th to mid-10th century) a settlement was established within a moated enclosure, of which the Erft formed one side. Its interior, surrounded by a palisade, was slightly heightened by layers of brushwood, sand and clay.At least five timber buildings were arranged about a central area in which lay a well and a rubbish-pit. A wooden bridge gave access to the enclosure from the east.

Periods II and III are archaeologically the most crucial. In Period II (second half of 10th century) the original enclosure, which here will be called Enclosure A, was remodelled. In plan it became a rounded oblong, and its interior was irregularly banked-up to heights varying from 1 to 1½ metres above the earlier level. Although a new house was set on one of the highest points, and some of the earlier houses were removed, it is noteworthy that the principal building of Period I is shown to have remained in position. A second enclosure (B) was added, connected to the first by a causeway reserved during the cutting of the new eastern ditch. It is to this period that Herrnbrodt attaches the label '*Kernmotte* '.

But it is in Period III (11th to mid-13th centuries) that we see a really decisive transformation. The typical plan of the fully-evolved motte-and-bailey is imposed on the site: there is a motte which is a truncated cone, and a bailey unimpeachably lunate. But it is Dr Herrnbrodt's purpose to show that this was not the result of a simple transformation, as it appears to be, but of a series of developments—a gradual evolution. For each of the layers of which thenceforth the motte was composed is held to represent a sub-period. The layer attributed to Period III A heightens the '*Kernmotte*' by about a metre. The Period III B layer adds roughly 3 m. more. Period III C-D is represented by minor heightenings and makings-good such as British mottes (manifestly not evolving) have revealed.

Period IV (*Suburbanum*) covers the occupation of the site from the mid-13th century onward. It is unremarkable and will be passed over here.

There are two cruxes to be considered: first the nature and implications of the earthwork changes from Period I to Period II; and secondly the interpretation of the layers making up the Period III *Hochmotte*.

In Period II the ditch of Enclosure A, as it appears on the drawn section of *Faltblatt* 9, seems incapable of having furnished all the material of the '*Kernmotte*'. This implies one of two procedures atypical of the motte. The extra soil might have been imported, as has shown to be the case on several late-medieval moated sites in England. Otherwise, it must be regarded as the product of repeated cleanings or re-cuttings of the surrounding ditch, heaped up conveniently near the places from which it was gradually derived. On this site, silting-up of the ditch must have been constant and severe, and the contour-plan of the '*Kernmotte*' shows that its highest points are ridges neighbouring those sections of the ditch which issue directly from the river. There has been no attempt to make a central mound—on the contrary, there is a median hollow. Need the developments of Period II be more than a local response to heavy silting and a rising water-table? That the principal building of Period I was retained in Period II argues for continuity in the nature and use of the site. If any cropping or grazing was required to continue on the site, the addition of Enclosure B is sufficiently explained; for the superimposition of raw soil on Enclosure A would virtually have sterilized it (the drawn section shows the old ground surface buried beneath the added soil), and Enclosure B may represent no more than alternative provision of enclosed grazing land. Arguments that it marks military or social segregation are hardly justifiable in view of the fact that no buildings were discovered in Enclosure B.

Turning to Period III A we are suddenly faced with the typical plan of a fully-evolved motte-and-bailey. Since the layer labelled III A adds only a metre to the mound, and leaves it lacking about $3\frac{1}{2}$ m. of its ultimate height, it would seem that the layout in plan has anticipated the development of its characteristic central feature.

Layer III A does not cover the main building common to Periods I and II, and this—over 100 years old—is accordingly held to have been preserved for further use while the rest of the site was so decisively changed; but it disappears from the plan when the next layer (III B) is added to the pile, and thence forward the motte is shown destitute of buildings. It is remarkable that, if each layer does represent a distinct structural phase, at no stage is there any sign of the special buildings which we might expect the mound to have supported during each sub-period. Dr Herrnbrodt's skill would certainly have disclosed them, even had they been methodically dismantled stage by stage; and it should be noted that the buildings of Periods I and II were conspicuously not eradicated—their remains were left to be embedded in the mound. It is true, however, that the conditions which preserved the actual timbers of these early buildings would not obtain in the upper layers of the motte. That a tower crowned the motte in its final form is conceded by the reconstruction-drawing of Period III, though the structure shown is interpolated from the

Hoverberg motte. It seems that damage to the top of the mound removed all structural indications; hence the tower (like the buildings in the bailey) does not appear on the plans recording the excavated structures.

It must be made clear that the reviewer did not see this site while it was under excavation, and can judge only from the published material. This offers little scope for assessment of the soil problems. The small scale of the published sections (1 : 200) necessarily involves some simplification of the evidence, and comparison of the photograph of pl. 22 with any of the drawn sections indicates its degree. Moreover, no key is given to the conventions used. The schematism of the drawn sections is further demonstrated by such details as the relationship of *Graben* I and *Graben* II in Profile (b) of *Faltblatt* 10. While among the photographs there are excellent details of surviving timberwork, the absence of scales robs them of immediacy; and the photographic demonstration of the overall stratification—which is, after all, the crucial matter—leaves much to be desired. A general index would be a welcome addition to the whole.

Dr Adelhart Zippelius contributes a section on the reconstruction of the important timber structures, with a hypothetical scheme for the evolution of stave-construction. The reviewer agrees that palisade-construction is relevant to its beginnings, but the transition from the foundation-trench to the ground-sill in a shallow slot seems to require the intervening development of a longitudinal tie at the base, which is not postulated in this scheme. Dr Zippelius would have the Abinger motte's tower as a completely enclosed stave-construction, on the basis of the slots or mortices in two of its cornerposts. It may indeed have been so. It has been explicitly stated elsewhere that the cramped interior of the Abinger motte's palisaded top merely *suggested* the stilted-tower hypothesis, which other evidence showed to have general substance. Here, it need only be pointed out that the Abinger tower was based in a series of separate post-holes, not in a trench, and that the slots or mortices in the corner-posts were certainly not interconnected by even the thinnest wall-line at the level at which they were revealed (probably over a foot below the original surface). It follows that the slots in the timbers were empty below ground and that probably the timbers were slotted uniformly from end to end, ' stops ' being introduced where they were needed during the construction of the tower. Thus we cannot possibly determine in this case whether the whole or only a part of the tower was enclosed by horizontal or vertical planking. Moreover, there can be no certainty that these two timbers were not re-used material from the earlier tower.

The ultimate effect of *Der Husterknupp* on our views of the nature and origins of mottes is difficult to predict. We need a multiplication of instances to distinguish the local and particular from the fundamental and general. That a motte of the 11th century sits on top of an earlier earthwork of different form need not imply that the one was evolved from the other. At More in Shropshire, last year, the reviewer briefly examined a motte which appears to have been erected over an earlier ringwork of post-Conquest date. This could not be claimed as evidence of independent evolution in the 12th century. It indicates merely that choice was exercised as to which of two or more familiar forms should be used at a particular time and place. It has yet to be shown that the builders of the Husterknupp did not have a similar range of choice in Periods II and III. The assured way in which the typical motte-and-bailey plan of Period III was imposed on the site adds to the probability that fully-evolved motte-and-bailey castles existed elsewhere at this time (early 11th century).

We find not one but at least three distinctive earthwork forms in the repertoire of the Normans. First, there is the type of motte thought to have been specially associated with the use of tall towers. Secondly, there is the lower, broader motte capable of carrying a hall and other domestic buildings. Thirdly, there is whatever is meant by the question-

begging, self-contradictory term *ring-motte*. Mr Jope, Mr Alcock and the reviewer have each independently reached the conclusion that this term must be discarded in favour of ring-*work*. One man's rath is another man's ring-motte, and a ring-motte is not a motte at all.

It is the ringwork which can most readily be related to earlier medieval fortification. The earth-and-timber ramparts of the *Burgwalle* and the Viking fortresses notably represent the form (the latter instance may not be irrelevant to its reappearance in Norman hands, and the name ring-motte may have blinded us to the possibility that some of the works it covered might be our ' missing ' Viking sites). The distinction between the most heavily embanked ringworks of known Norman date and the lowest of the residential mottes is sometimes a fine one. It is not impossible that a variant development of theringwork, perhaps, akin to Dr Herrnbrodt's '*Kernmotte*', was the basis of the residential ' hall-motte '; an evolution which could well have been stimulated by an independent tradition of watch-tower mottes. There are elements of continuity in the basic character of the Husterknupp settlement from the 9th to the 11th century; but then the residential development of Enclosure A was ended. In one blow the typical plan of the motte-and-bailey was substituted, and it is agreed that the motte probably carried a watch-tower. Whatever the significance of the developments of Period II, the sophistication and assurance of the plan imposed in Period III were surely achieved in the course of its independent development and use elsewhere.

Der Husterknupp is likely to become an archaeological classic, not least because the timber buildings of Periods I and II are of special interest and importance. Dr Herrnbrodt and his colleagues deserve our warmest congratulations on their achievement.

BRIAN HOPE-TAYLOR

ZÁKLADY SLOVANSKÉHO ŽELEZÁŘSKÉHO HUTNICTVÍ V ČESKYCH ZEMÍCH (Die Grundlagen der Slavischen Eisenindustrie in der Böhmischen Ländern). Československá Akademie věd Monumenta Archaeologica. VI. *By* RADOMÍR PLEINER. *Prague*, 1958. *pp.* 336, 32 *plates*, *five folded maps in back cover*, 68 *text figures*. 49 *Kčs.*

To those who follow scientific research upon iron technology Dr Pleiner's work will require no introduction. The present volume is the result of years of research into the problems of the iron industry from prehistoric times up to the 12th century A.D. In his various chapters the author treats of the rise and development of the ancient direct method of iron production by the bloomery process: Methods of mining and the preparation of the raw material for smelting: The beginning of iron working in Bohemia and Moravia during the Hallstatt period: Iron metallurgy and its general influence during the La Tène period: The industry in Roman times: The beginning of Slavonic iron working and its continuation to early feudal times: and finally, the development of iron metallurgy in Bohemia and Moravia up to the 12th century A.D.

At our present state of knowledge of the history of iron, and in particular that of prehistoric and Roman iron metallurgy, there are two problems about which we require to know more. Firstly, it has to be admitted that our knowledge of the iron smelting furnace is not complete. Here, Dr Pleiner renders invaluable service by placing before us a fine series of furnaces with full documentation and closely reasoned metallurgical discussion. It is only through such work that true progress can be made. In this connection, Professor Bertolone's initiative for an international Corpus of smelting furnaces is to be welcomed as a step in the right direction. The first part of this Corpus, entitled Ancient Metallurgical Furnaces in Great Britain to the end of the Roman Occupation, by T. K. Penniman,

I. M. Allen and Anthony Wootton, has now been published in *Sibrium*, and surveys the smelting furnaces so far known in this country.

For his demonstration of the evolution and technology of the iron furnace, Dr Pleiner has been fortunate in having rich material to work upon, and he has therefore been able to establish a satisfactory series. Naturally, the early Hallstatt furnaces are not well documented, but in the regions under discussion they appear to have been bowl furnaces of very simple form, probably with forced draught furnished by a bellows. In the following La Tène period very considerable advance in furnace technology is to be observed. A somewhat dome-like furnace with shallow hearth, and wind passage, is noted. Also, a small shaft-like furnace with deep hearth which is clearly designed to aid carburization, after smelting, of the bloom within the furnace itself. Scientific examination of slags leads Dr Pleiner to the conclusion that high temperatures, of the order of 1300°–1350° C, were attained in these furnaces. On this point, the author's discussion is of vital importance. In Roman times three major furnace types are present:

(*a*) A pronounced La Tène type of small shaft furnace with deeply sunk hearth and not very high superstructure. Draught was probably by simple bellows.

(*b*) The second type which is particularly characteristic of the Roman period is a free standing shaft furnace without sunk hearth, and developed from the La Tène furnace. Such furnaces may have natural or forced draught.

(*c*) The third type is a peculiar furnace excavated below ground level in the form of an inverted cone. These furnaces are arranged to be worked with a ' Vorofen ' or front hearth.

Secondly, we have still too little knowledge of the place of steel, or a steel-like structure, within the iron industry. Also, of its impact upon the general rise and development of that industry. Finery furnaces and forges are of course known in which the wrought iron bloom may be more or less carburized, but prior to the present work we have had little or no sound evidence to show the purposeful production of a steel-like material directly from the furnace itself. The author's research into furnace construction and technique of operation now make it probable that in certain protohistoric smelting furnaces the construction allowed the production of iron blooms which carried zones of hypoeutectoid steel.

It is not possible to mention the many other matters of interest with which Dr Pleiner deals in this substantial volume such as the techniques of the smith, his tools, and iron weapons produced. This book is to be recommended without hesitation. Indeed, it is essential to the engineer or technologist working upon problems of the early iron industry in Europe. Also, study of the book will be of great benefit to excavators who may have the good fortune to discover simple or complex metallurgical furnaces. Again, while the major part of the work is devoted to the scientific and technological theme, the archaeological background of the various iron using cultures has not been neglected.

The production of the book is very good. The plates are large and clear, and the photographic reproduction excellent. There are also numerous line drawings and diagrams in the text: these are of ample size and clear to read. To the archaeologist, a series of fine coloured distribution maps will be most useful. Captions to the plates and text figures are given in German as well as Czech. An adequate German summary is provided. From the point of view of the British reader there is a difficulty. Dr Pleiner's text is, of course, in Czech. In his editorial, ANTIQUITY, XXXIII, no. 132, 1959, p. 240, Dr Glyn Daniel has aptly commented upon this language difficulty. It is indeed good news to learn of the most public-spirited action by the Peabody Museum of Archaeology and Ethnology at Harvard who have decided to issue a series of translations from the Russian of important selected books and articles dealing with prehistoric archaeology and physical anthropology. Now

that the vital role of the metals in pre- and protohistory is at last fully accepted, may a plea be entered for the inclusion of Czech as well as the Russian language, and for Dr Pleiner's book in particular. Its value is such that it is, without question, most worthy of a full translation.

H. H. COGHLAN

EXCAVATIONS AT JERICHO. I. THE TOMBS EXCAVATED IN 1953–54. *By* K. M. KENYON *et al. British School of Archaeology in Jerusalem*, 1960. *pp.* xxii + 542, 230 *text figures*, 43 *plates*. 6 *gns.*

This is the first volume of the final report of the excavations conducted by Miss K. M. Kenyon at Jericho in 1952–58. It is a perfect model of archaeological publication: a complete and precise presentation of all the evidence, with numerous plans and drawings and good photographs. And the interpretation is sapiently argued: the archaeological 'facts' are explained (as far as possible) by human and logical reasons and seen as elements in a civilization and an history. This reviewer is happy to express his agreement with the main conclusions of the author.

Four tombs are assigned to the 'Proto-Urban' Period. This is a new name, coined by Miss Kenyon instead of the usual nomenclature of both 'Upper Chalcolithic' and 'EB Ia'. There is much to be said in favour of this innovation: as the reviewer has already shown from his finds at Tell el-Fâr'ah, 'Upper Chalcolithic' and 'EB Ia' are contemporaneous; both cultures are to be compared with the Proto-Dynastic Period in Egypt and the Proto-Literate Age in Mesopotamia; they precede the walled cities of the Early Bronze Age and the widespread urban development in Palestine. But I would hesitate to accept Miss Kenyon's conclusion that 'this stage is the formative one on which the Early Bronze Age developments were based', p. 10. From the evidence of the pottery alone, it may be true for 'EB Ia' at Jericho (comp. Tombs A 13 upper levels and A 108), at Ay, Tell en-Nasbeh and elsewhere in the South, it is not true, or at any rate it is less true, at Tell el-Fâr'ah and other Northern sites, where 'EB Ia' is missing. With regard to architecture and general culture, it is not upheld by the evidence either for 'EB Ia' or for 'Upper Chalcolithic': at this period, Jericho, Ay, Tell en-Nasbeh . . . as well as Tell el-Far'ah, Megiddo, Beisan . . . are 'villages' with poor structures or no structures at all, only huts and pits. Transition to the 'towns' of Early Bronze is better explained by the arrival of a new population and the introduction of a new culture than by evolution from the preceding stage. So that, though satisfied with the name 'Proto-Urban', I wonder if 'Pre-Urban' would not be even better.

Altogether, nine tombs of Early Bronze Age were found, which is a strangely small number. They cover the whole period, nearly one millennium. Their chronological order seems well established by the author, except that I would put T. A 127 under EB II (at least overlapping into EB II) rather than under EB I, and that T. F 5, described without commentary between T. F 2 and T. A 114 (both of EB III) seems to me clearly EB II.

Much more numerous (248 tombs) and more interesting are the tombs described under the heading 'Intermediate Early Bronze-Middle Bronze Period'. This again is a new name, and fully justified. It should replace the name 'Middle Bronze I' (Beit Mirsim I–H), hitherto applied to a culture which has nothing to do with Middle Bronze proper. In comparison to Early Bronze also, it represents a complete break in all respects, pottery, architecture (or lack of architecture) and burial customs. The tombs are individual burials, quite distinct from the multiple EB burials and the MB family tombs. They fall into five groups, according to shape, disposition of the skeleton, funerary equipment: (1) Dagger type: small, neatly cut tombs, containing intact crouching burials; typical offering: one dagger, no pots; (2) Pottery type: large tombs with wide deep shafts, roughly cut, in which the

skeleton was placed disarticulated; offerings: only pots (varying from one to nine) of a limited range of form; (3) Square-shaft type: tomb-chamber resembling that of the Dagger type, with a shaft square in plan, containing intact crouching burials; offerings: both daggers and pots; (4) Bead type: poorly cut and shapeless tombs in which the skeletal remains are scattered; offerings: only beads and small bronze objects such as earrings; (5) Outsize type: chamber and shaft even greater than in the Pottery type, skeleton often disarticulated; offerings: a number of pots (up to twenty-two), also some daggers and javelins. There is no way of arranging these types in chronological order; the stage reflects the incursion into Palestine of a number of nomadic or semi-nomadic groups, with slightly different burial customs. They destroyed the urban civilization of the Early Bronze Age, camped on the ruins of the Tell of Jericho as they did elsewhere and, only after a long time, began to build houses. They were a race of shepherds in the stage of settling themselves and desirous of providing for their dead a habitation more enduring than that of the living. Historically, they belong to the Amorite migration of the end of the IIIrd millennium B.C. It is in part contemporaneous with the first Intermediate Period in Egypt and this is an additional motive for adopting the name proposed by Miss Kenyon.

The entire second half of this bulky volume is concerned with the Middle Bronze Age tombs, but this review will pass over this section more quickly because the period is better known. Three points only will be selected. (*a*) *Burial customs:* careful excavation brought to light many interesting facts, such as the way of disposing of the bones and offerings from earlier burials, the opening and refilling of the shaft for each burial; in six tombs a final burial of a number of corpses is explained not as a ritual practice or as the result of a slaughter when the Middle Bronze town was destroyed, but by some sort of disease which was responsible for the death of entire families. One tomb, exceptionally, is individual and contains the skeleton of a young warrior, with the same equipment as T. A in Tell el-Fâr'ah. (*b*) *Relative chronology:* according to a thorough study of the pottery, the tombs are classified in five successive groups. Two points are striking: MB II A (= MB I in Miss Kenyon's nomenclature) is missing, which may confirm observations made in other sites (for instance at Tell el-Fâr'ah) showing that Middle Bronze had a rather modest beginning; in particular, the ramparts, which are taken as a characteristic of that period, were not built at the outset. The second point is that, throughout the five groups delimited, the furniture and pottery are equally rich and very homogeneous; that signifies economic prosperity and political stability and must be taken into account for a just appreciation of the ' Hyksos Period ' in Palestine, which covers approximately MB II (MB II A of others) and which should not be judged only from the Egyptian angle. (*c*) *Exceptional preservation* of organic materials, due to some accumulation of gas which killed the micro-organisms before the process of decay was complete—not only wood but also basketry, textiles, fruit and flesh were preserved. It is an unequalled stroke of good fortune for a Palestinian archaeologist, and Miss Kenyon has made the best of it.

Not only archaeologists, but anyone studying the ancient civilization and history of Palestine, will find this book invaluable.

R. DE VAUX, O.P.

STUDIES IN THE EARLY BRITISH CHURCH. *By* NORA K. CHADWICK, KATHLEEN HUGHES, CHRISTOPHER BROOKE, KENNETH JACKSON. *Cambridge University Press*, 1958. *pp.* 375. 45*s.*

This important collection of six essays by four Cambridge specialists interested in early British history is concerned with the intellectual life of the ' Church of the British or Welsh-speaking area during the period between the " age of the saints " in the sixth century

and the changes brought about by the coming of the Normans and the medieval religious Orders '.

Mrs Nora Chadwick, who contributes two of the essays, has provided an introduction in which she explains the nature and scope of the studies, discusses the classes of evidence available, brings together the main findings of the six studies and expresses her views on the ' crippling ' effects of ' over-intensive and exclusive specialisation ' in studying the history of the period. Mrs Chadwick's first essay is on ' Early Culture and Learning in North Wales '. In this, she examines the lively antiquarian speculation and acute ' national ' sense of the 9th century which produced, among other things, the origin legends concerning the ' intrusive ' ruling dynasties. She argues (not altogether convincingly to my mind) that the tradition of Cunedda's migration to Wales ' belongs in all its essential features to the same class as those related of the Picts and the rulers of Scottish Dálriada'. Among the centres of learning which propagated these legends were Abernethy, Brechin perhaps, and Bangor, co. Down. There was doubtless a ' local intellectual centre ' in the 9th century in western Britain and Mrs Chadwick makes a patient attempt to locate it by examining the evidence of the Latin texts of Welsh provenance: Nennius and the ' Historia Brittonum ', the ' Annales Cambriae '—with their significant Irish entries and their North-British entries—and the genealogies; all these are carefully analysed. She observes that relationship of the entries in ' Annales Cambriae ' up to the beginning of the 7th century ' to Irish ecclesiastical affairs, as well as to Irish annalistic writings, is very clear ', and she argues that most of these entries are derived from a common source. Where, then, were the entries relating to North Britain and to Arthur combined with the common source of the Irish annalistic entries? Mrs Chadwick decides against Iona and, rather surprisingly, against North Wales; she thinks Whithorn was a more likely centre—or Hoddom, perhaps. In her view, the internal evidence of the closing chapters of Nennius and of the 'Annales Cambriae' points to a 7th-century document from Strathclyde, and she makes the stimulating conjecture that the earliest records of Cumbria may have been compiled at Carlisle by Rhun, son of Urien.

In the 9th century there was a new dynasty in Gwynedd, and the courts of Merfyn Frych and Rhodri Mawr made the region an active cultural centre where native oral tradition, especially that concerning North Britain, and Latin learning were effectively fused in written form. In 1952, R. Derolez (*L'Antiquité Classique*, XXI, 374) indicated the importance of Wales ' as a station on the " Scottish " exiles' road to the continent ', and Mrs Chadwick gives a lucid and exciting account of the influence of the royal court of Gwynedd at a time when ' Rhodri and his sons were aiming at a reunited British people and an ultimate Saxon conquest '. Gwynedd was not only casting its memory back to North Britain but also looking across the seas both towards the learning of Ireland and towards the culture of the Merovingian court. The documents show the fruits of the lively interaction of these cultural relationships.

Mrs Chadwick's third contribution is an account of ' Intellectual Life in West Wales in the Last Days of the Celtic Church ', and here she brings a fresh emphasis on the importance of the *clas* at Llanbadarn Fawr under Sulien and his remarkable sons as a meeting point of Celtic and Latin learning. She brings together what is known about the final struggle over the issues which in differing forms had for long caused strain and tension between the church in western Britain and the *Unitas Catholica*. South-west Wales still maintained close connexions with southern Ireland and Mrs Chadwick illustrates how these show their influence, for example, in Rhigyfarch's *Life* of St David. More significantly, Llanbadarn valiantly tried to turn hagiography into ecclesiastical (and therefore inevitably political) propaganda. Mrs Chadwick's account of Sulien's role as peacemaker between William the

Conquerer and Rhys ap Tewdwr and Gruffudd ap Cynan in 1081 and the subsequent prominence of St David's is imaginative and on the whole persuasive.

These three contributions show detailed knowledge of the sources, imaginative insight and an enviable gift for synthesis. Occasionally, there are points of detail and minor inconsistencies which call for correction: e.g. the letter from Cummian to Segene is said to have been ' written as is believed *c.* 627 ' (p. 14), but—correctly—' probably *c.* 632–3 ' (p. 51); the misreading of Bede's ' regulariter fidem et mysteria veritatis edoctus ' as ' to be consecrated *regulariter* ' (p. 27) is made right on p. 128 in another context; p. 40, read Vatican Reg. 1964 (the error is probably due to Kenney, p. 154); p. 40, n. 2, on Gunn, cf. Mommsen's ' librum expressit non optime W. Gunn '; p. 41, van Hamel really argued that BL has *two* recensions of *Lebor Bretnach*—L^1 and L^2: L^2 and L^3 ' constitute in fact but one version ', but ' for convenience' sake ' he continued to distinguish three versions; p. 43, the reference to Fernmail is HB. *c.* 49; p. 48, n. 6 on ' Gwŷr y Gogledd ', add A. W. Wade-Evans, *Arch. Camb.*, 1930, 339 f.; p. 49, the reference to ' MS Hengwrt 536 ' is some decades out of date—read Peniarth MS. 45 (NLW); p. 93, the Cadfan stone at Llangadwaladr does not form ' the lintel inside the north door of the church '—it is built into the north wall of the nave; p. 95, it should be noted that the Bamburg cryptogram, line 27, also gives ' Suadbar scripsit '; p. 104, n. 3, read Kenney p. 596 (no. 411); p. 105, n. 2 for the Zimmer reference, read Hellmann, *Sedulius Scottus* (1906), 148, n. 5, and Stern, *ZCP*, VI, (1908), 551 n.; p. 113, n. 2 is too summary a statement of Sir Ifor Williams's view—see his article s.n. *Gwrtheyrn* in *Dictionary of Welsh Biography;* p. 114, Hywel Dda's supporters would not have been active in the 9th century; pp. 115–6, read *(H)elias* for Elian; p. 126, n. 4, add Robin Flower, *NLW Journal*, II (1940), 7–14.

Dr Kathleen Hughes in her first study examines the purpose and provenance of the important BM. MS. Cotton Vespasian A. XIV ' Vitae Sanctorum Wallensium ' of about 1200, and discusses *inter alia* the stimulating theories propounded by the Rev. Silas Harris in *Journal Hist. Soc. Church in Wales*, III (1953), 3–53. She concludes that during the first half of the 12th century ' material from the hagiographical schools of Llanbadarn Fawr . . . and possibly from Llandaff was gathered together at Gloucester ' and copied about 1200 into Vespasian A. XIV. Miss Hughes provisionally agrees with Mr Harris's claim that the monks of the priory of Saint-Florent at Monmouth compiled the manuscript. She has argued a sound case for the view that ' both the contents and the palaeography suggest that Vespasian A. XIV may have been considered as a source-book for historical record ', and that it ' is another illustration of the interest which the Anglo-Norman settlers felt in the pseudo-historical traditions of the Celtic Past'.

In ' The Archbishops of St David's, Llandaff and Caerleon-on-Usk ', Professor Christopher Brooke brings vigorous zest and much learning to deal with the complicated problems presented by the *Book of Llandaff* (*c.* 1135–50) and the rivalry between St David's and Llandaff. He discerns ' some link ' between the work and Geoffrey of Monmouth's *Historia*; he suggests that Rhigyfarch was aware of the ' fabrications ' that were being skilfully executed at Canterbury; he says that ' Urban's *familia* forged a book—the *Liber Landavensis*; they may also have forged a diocese '; that ' the charters are a classic statement of the principle of fake diplomatic '; that ' the technique . . . entitles the author to a high place among 12th-century antiquaries as a student of historical method '. Who, then, was this professional hagiographer and professional forger of the tribe of the prolific falsifier Guerno ? Professor Brooke's opinion is that it was Caradog of Llancarfan, but he would not be dissatisfied with Geoffrey of Monmouth. It is too early to assess the full value of this brilliantly exciting and challenging essay (with its well-packed appendices). There is much more detailed work to be done on the sources to establish the nature and amount of the

genuine elements derived from earlier grants and *diplomata* and adapted for the purposes of a later diplomatic—in particular there must be a careful examination of the Llancarfan ' cartulary '; there is also a need for more patient collating of annals and genealogies to establish as exact a chronology as possible; and it is always important to bear in mind the fluctuations of the limits of episcopal ' authority ' in Wales before the Norman diocesan pattern was arbitrarily imposed there. Meanwhile, one or two comments may be offered on details in Professor Brooke's essay: p. 221, n. 1, there are good reasons for not adducing the *teliau testis* of the Lichfield entry as ' a striking example ' of ' the practice of making grants to the patron saint of a church as to a living person '; p. 226, n. 2, ' Llandeilo Fawr (Carm.) ' is *not* ' given as one of the seven " bishop's seats " in Dyfed ' in the list given by Haddan and Stubbs—see further Wade-Evans, *Welsh Medieval Law*, 121, and Dr Hughes's note, p. 271; p. 228–9, n. 2, *Brut y Tywysogion* in fact says ' Vchdrut, bishop of Llan Daf . . . died . . . And after him Nicol vab Gwrgant became bishop '; p. 238 (2) (*a*), the references to Jacob ' abbot of Llandough ' are inaccurate.

Dr Kathleen Hughes's second essay is concerned with the distribution of Irish scriptoria and centres of learning from A.D. 730 to 1111. Because ' the palaeographical evidence is technical, controversial and scanty ' she relies on the evidence of the *obits* in the Irish annals. From the late 8th to the early 10th centuries, the main concentration was in central Ireland and on the coasts. The scribal activity was doubtless stimulated by intellectual and spiritual vigour, but its flourishing depended on ' financial ' stability: without economic security learned tradition declined—Duleek and Bangor are cases in point. In her account of the anchoritic revival Dr Hughes is right in warning us against too ready an identification of scribes and anchorites: ' the evidence suggests that the anchorites became scribes through their association with the great scriptoria of the period and that the majority of the anchoritic settlements developed no distinguished scribal traditions.' During the late 10th and 11th centuries the deaths of poets and historians are more frequently noticed in the annals, and there are many references to distinction and achievement in the native culture. The western centres in Munster and on the Shannon attain greater importance. Dr Hughes has some pertinent things to say about the interpretation of the rather meagre evidence relating to Welsh monastic scriptoria. A small point: on the ' double purpose of *lex* ' (p. 251) and the two meanings of Irish *cáin*—' tribute ' and ' law ', cf. Dr Binchy's comment, *Críth Gablach*, 79—' the wider meaning of " written law " is undoubtedly derived from the numerous monastic ordinances issued in the 8th century ', and see also Thurneysen, *ZCP*, XVIII (1930), 396.

Finally, Professor Kenneth Jackson examines the sources for the *Life* of St Kentigern. The author of the 12th century ' Herbertian ' fragmentary *Life*, a ' clericus Sancti Kentegerni ' (probably a Norman priest) made use of written and oral sources. Later in the same century when Jocelyn of Furness compiled a new *Vita* he used both the ' Herbertian ' *Life* (writing rather disparagingly of its style and doctrine—especially the ' Fatherless Child ' motif which the anonymous cleric seems to have introduced) and at least one of the written sources on which the anonymous *Life* had been based. Having given sound reasons for not thinking that the written source was in Gaelic, Professor Jackson suggests that the ' " little volume " in Scottic Latin was a product of the Gaelic occupation of Strathclyde in the eleventh century ' and that it survived at Glasgow until the days of the Herbertian author. The Offices of St Kentigern in the 12th century Sprouston Breviary (NLS. MS. 18.2.13*b*) and in the 16th century Breviary of Aberdeen are ' mixed ' summaries. Both in the essay and in an appendix Professor Jackson gives a valuable ' form and language ' analysis of the Lives and Offices; the illuminating examination of the Welsh episode (Jocelyn *c.* 23–31), for example, makes it clear that Jocelyn's account is doubtless derived from a

12th-century Norman-Latin document compiled in North Wales; again, the assigning of a see to Kentigern at Hoddom ' is a late and spurious invention '. There are indications of Cumbric linguistic forms, and Professor Jackson tentatively suggests that one of the lost basic sources was an 11th-century ' Vita Kentegirni ' written in a Cumbric context in Strathclyde; this formed the core of the written source of Gaelic provenance used both by the Herbertian author and by Jocelyn, but this ' Gaelic ' work had accretions derived from a Kentigern—Servanus compilation, also of Gaelic provenance, and from other Gaelic material; there was yet another source which the Herbertian author probably drew upon—a Lothian document, Brittonic in context, compiled in all likelihood, as Professor Jackson shrewdly surmises, by a cleric who was ' a member of the Gaelic overlord class which held Lothian in the eleventh and twelfth centuries . . . but . . . in touch with the local and probably oral lore of a Brittonic peasantry.' Although Professor Jackson concludes that the *Life* of St Kentigern is of little value as a reliable historical source for events in Scotland in the 5th and 6th centuries (apart from the interesting indication that in Strathclyde ' the kings were " subject " to the bishop as long as the kingdom lasted '), his essay is of major importance and significance.

I. Ll. Foster

ARCHAEOLOGICAL EXCAVATIONS AT JAMESTOWN, VIRGINIA. *By* John L. Cotter. *pp.* x + 299, 28 *figures and map in pocket*, 92 *plates. National Park Service: Archaeological Research Series No.* 4. *U.S. Dept of the Interior, Washington*, 1958. $2.75.

Jamestown, the first permanent English settlement in America and originally the capital of Virginia, should for this reason alone arouse interest on both sides of the Atlantic. It had an unfortunate history. It had troubles with the Indians, famine, fire and civil strife, it was built in an unhealthy situation, and once the Colony was well established the planters preferred to live outside it on their estates, so, to use the author's words, ' it was never a successful integrated community '. The capital was eventually moved to Williamsburg, and Jamestown was finally abandoned. Even this did not end the troubles of the site, because it suffered erosion by the James River and the losses seem to have included the foundations of the original fort of 1607. All that remains above ground is part of the church tower and of one of the later brick houses.

Although the outlines of its history are well known, we know little from documentary sources of the appearance of the place or the daily life of its inhabitants, and the help of archaeology was badly needed to clothe the skeleton. Sporadic explorations began at the end of the last century, and scientific excavations on a large scale started when the National Park Service took over the site in 1934. The work culminated in Dr Cotter's extensive excavations in 1954–6, which were prompted by the 350th Anniversary Celebrations of the State of Virginia in 1957. This admirable report contains an account of all previous explorations as well as his own.

The site included relatively few substantial brick structures, far fewer than historical sources might have led us to expect, and it seems that frame houses predominated. This is not altogether surprising, since many an East Anglian village would yield a similar pattern even now. In addition to the houses there was a church, which was rebuilt several times, brick, lime and pottery kilns, possible metal-working pits, a glassworks which had little success, and such interesting trifles as an ice house and a brew- or bake-house. Not only are we given admirable plans and photographs of these features, but also conjectural reconstructions of some of them in the form of photographs of paintings.

The European reader will probably find that the most interesting part of the report is

the information about the smaller finds on the site, illustrated by a good series of plates, and his admiration will be kindled by the immense amount of research which has gone into the establishment of their origins. Alongside locally-made earthenware, he will find European types with which he is familiar and others which he ought to know better than he does! There are Bellarmine jugs, Portuguese majolica, German salt glazes, English slipwares and ' delftwares ', and some nice examples of sgraffito ware from Barnstaple. There are familiar-looking wine bottles, some of them with seals, clay pipes, jettons, buckles, horseshoes, a brown bill, and numerous tools, including axes showing late survivals of medieval types. These well-dated examples make the work a useful reference book for museum men in this country.

Being primarily a factual archaeological report, this is not meant to be light reading, but it serves its purpose of filling out the historical picture and does not lack human touches. From the appendix on Flora and Fauna, a list compiled from contemporary records with scientific names added by members of the Smithsonian Institution staff, this reviewer at any rate is glad to learn that, among all their other trials, the inhabitants of Jamestown were spared the ticks, ' presumably a recent import ', which were, he infers, a grief to the excavators.

G. H. S. Bushnell

ROCK PAINTINGS OF THE SOUTH-WEST CAPE. *Compiled by* Townley Johnson, Hyme Rabinowitz and Percy Sieff. *Nasionale Boekhoudel BPK, Cape Town*, 1959. *pp.* 88, 40 *pp. of bilingual text, with line illustrations;* 48 *pages of two-colour plates on art paper.* 25*s.*

The past six months have seen the appearance of three new volumes on Southern African rock art: *Rock Art of the Federation* edited by R. Summers, *Anibib and Omandumba* by the Abbé Breuil, and the present work. Before the former two giants the latter appears as a very insignificant thing, but despite its small size it is none the less of considerable merit. The authors do not claim to have produced an archaeological treatise on what is, in fact, valuable archaeological evidence. Their purpose, clearly stated, was to make some record of these fascinating paintings before they are lost for all time (as some now are) by natural deterioration or human interference, and, in their own words, ' to let others share in the excitement of discovery and the pleasure which an appreciation of the art of the rock-painter engenders.' In these respects the authors have undoubtedly succeeded in their purpose.

The archaeologist interested in Africa's past will find much of value despite the fact that the book is intended primarily for the non-specialist reader. It is unfortunate that the authors should have referred to pl. 41 as ' Egyptian-like stylized paintings ', as this kind of remark (albeit quite unintentionally) frequently leads to quite untrue and wildly romantic notions about the origin of the art and its proper associations.

The captions to the illustrations are all too often rather trite, and might easily have been expanded somewhat without transgressing the bounds of legitimate interpretation. The ' strange appendage ' on the tail of the elephant (p. 24) is no more than an exaggeration of the bristly tuft that actually exists there, and which has attracted the attention of other artists as far afield as the Lower Congo, and the Tassili massif.

The technique used for reproducing the paintings is new, and is described briefly. It would appear, however, to be costly in both labour and equipment, for a result which archaeologically has nothing to recommend it, and which, if desired for aesthetic reasons, could be achieved much more easily and cheaply by transferring the tracing to some suitably

mottled background, as Mr C. K. Cooke has done for so many of the Rhodesian paintings. The authors' experiments with various painting media are of great interest, and in some cases would seem to deal a death-blow to extravagent theories of armchair archaeologists; it should be borne in mind, however, that the successful experiments do not necessarily prove the identity of the original media.

The volume is slim, and not very well bound, and it is perhaps disappointing that the text is so brief (perhaps because it was deemed necessary or desirable to repeat these few words in Afrikaans). None the less it is an attractive and useful volume, enhanced by a valuable, if brief, foreword by the late Professor A. J. H. Goodwin.

R. R. INSKEEP

SUMMARY REPORT OF MODOC ROCK SHELTER, 1952, 1953, 1955, 1956. *By* MELVIN L. FOWLER. *pp.* 72, 20 *figures*, 8*vo. Illinois State Museum, Report of Investigations No.* 8. *Springfield, Illinois*, 1959.

The importance of the Modoc Rock Shelter is that investigations there first gave prominence to the possibility that the Archaic Stage in the United States, with its emphasis on food-gathering with hunting of small animals and birds, might have existed at a date when big game hunting was still widely practised. The dating was done by means of composite carbon samples assembled from small fragments scattered in the deposits, by methods developed by Dr F. Matson, and it gives a reasonably strong indication that the Archaic occupation of the shelter began about 8000 B.C., although there are several inconsistencies in detail in the series of dates.

There have been several earlier reports on this site, but the present one is useful since it brings our knowledge of it up to date and adds a discussion of the way in which Archaic peoples gradually became more expert and specialized in exploiting their environment on this and other sites in the same region. Thus it is deduced from the evidence that the initial occupation was ' on a foraging level ', utilizing all the resources of the area. After this, people appear to have settled down locally and exploited the special food resources of each smaller area; it is suggested that this process of adaptation culminated in seasonal cycles and the development of specialized camps for different seasons, reaching its peak about 3500 B.C.

The recognition of the importance of seasonal cycles is an important development in the study of the North American Archaic, and it is gratifying to note, from one of the publications quoted in this report, that the work of Grahame Clark at Star Carr has helped to make this recognition possible, and may indeed have stimulated it.

G. H. S. BUSHNELL

Book Chronicle—*continued from p.* 210

THE PREHISTORIC BACKGROUND OF INDIAN CULTURE. *By* D. H. GORDON. *pp.* 199, 32 *plates*, 25 *figures. Bombay, Bhulabhai Memorial Institute*, 1959, *London, Kegan Paul and Luzac.* 45*s.*

THE BUDDHIST ART OF GANDHĀRA. *By* SIR JOHN MARSHALL. *pp.* 118, 111 *plates. Cambridge, the University Press*, 1960. 45*s.* This is the first volume of the Memoirs of the Department of Archaeology in Pakistan, and is the late Sir John Marshall's account of the birth, growth and decline of the early Gandhāra school.

AFRICA'S PLACE IN THE EMERGENCE OF CIVILIZATION. *By* RAYMOND A. DART. *pp.* 96 *with many maps and diagrams. Johannesburg, The South African Broadcasting Corporation, P.O. Box* 8606, 1960. 1*s.* 0*d.* A brochure based on a series of six talks given in 1959 on the English Service of the South African Broadcasting Corporation under the title of the Van Riebeeck Lectures.

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